SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE CARNEGIE INSTITUTION.

TO THE EDITOR OF SCIENCE: I certainly appreciate your kind letter inviting me to join with you and others in publicly discussing in the columns of SCIENCE the question of the endowment of scientific research with special reference to the possibilities which are wrapped up in Mr. Carnegie's recent gift to the institution in Washington. Without such an invitation coming from you I should have hesitated to give utterance to any of the thoughts which naturally have arisen in my mind in this connection. I feel delicacy in making suggestions touching matters in reference to which my opinion has not been solicited. But when the editor of SCIENCE asks me to speak I cannot refuse to comply with his request.

There is but one truly scientific mind in the universe, whose vision sweeps from Sol to Alcyone, which notes the sparrows as they fall, and numbers the hairs of our heads. Every effort of the human intellect to ascertain the unknown as to the whole of things is an effort to apprehend the thought which lies in the great Synthetic Mind. As a philosopher I have long ago been taught the folly of calling anything great or anything little which Infinite Wisdom has planned and called into being. Nothing knowable is in certain aspects fundamentally more important than

any other thing which is knowable. When the astronomer undertakes, with the refinements of modern instrumental equipment, to photograph the visible heavens from pole to pole, to count the stars, and, if not naming them, to at least give them numerical designations, I respect his work, little as it bears to-day upon the practical life of man, because it is taking him and with him all mankind into communion with the all-creative Spirit. And when a friend of mine sits down to count the number of feathers which grow upon the belly of a duck, or another to trace the origin of the phylæ of the insect world through the bristles on the backs of larvæ, I feel for him in his laborious researches as profound a respect as I do for my astronomical friends with their vast and costly equipments. A fund given for the promotion of scientific research if well administered must be administered in the full consciousness of the fundamental fact that all knowledge is important and that all the sciences are but so many facets which bound the white diamond of eternal truth. The point which I mean to make is simply this, that the administration of a great fund like that established by Mr. Carnegie will ultimately fail of its aim unless those who are charged with the work are broad, learned and wise enough to avoid discriminating unduly in favor of one set of scientific investigators over against others. All the sciences should be treated impartially, and every honest worker seeking to add something to the sum of human knowledge should at least be treated sympathetically and aided, if possible, in the accomplishment of any feasible task. To devote the income of this great endowment to the promotion exclusively of a few things, the friends and advocates of which may be potent in argument, and influential by reason of personal acquaintance with those who are the administrators of the trust, would ultimately create in the minds of multitudes serious dissatisfaction. If I understand the attitude of the generous giver of this endowment it is an attitude of thorough impartiality toward the friends and advocates of scientific progress everywhere throughout the land.

In common with yourself, in view of what I have said I fear the result at the outset of the assumption by the Carnegie Institution of the control of existing agencies for research and for the education of students in research. To take up a few existing institutions and put them upon a satisfactory basis would be comparatively an easy matter, but such a course would inevitably in the end tie up the fund to the continued maintenance of such favored institutions, and the broader helpfulness of the fund would very probably be ultimately greatly impaired. Had I any voice in the administration of this fund I would plead for the avoidance at the outset of 'entangling alliances,' but I suppose that the wise men who have been selected to manage the affairs of this institution in Washington cannot fail to see the importance of this point.

As to various schemes which have been suggested of creating in the city of Washington an institution equipped with buildings and laboratories for the prosecution of special researches, I am inclined to think that such a course is, in view of all that already exists, of very doubtful expediency. There are already so many agencies for research at work which are not accomplishing all that might be expected from them, in many cases because of lack of sufficient resources, as to make it doubtful to my mind whether the creation of another supplementary piece of machinery promises as much as the application of lubricant to machinery already in existence. What is needed for the advancement of American science it seems to me is not multiplication

of agencies, but the nourishment and upbuilding of those which already exist. I say, without any fear of being successfully controverted, that there are already in America quite enough colleges, universities, scientific societies, laboratories and associations of scientific men. Scientific men should learn wisdom from men of affairs. This is the age of consolidation, in which the importance of union in effort is recognized. Plans now outrun accomplishment. Schemes for the accomplishment of the possible outnumber potent actualities. A hundred dream where one man acts. The land is full of abortive enterprises. The Carnegie Institution will do good just so far forth as it serves to be the fountain from which life-giving power shall be poured into those things that need strengthening lest they die. With this fund to create an institution which, by reason of its magnificent endowment, shall simply eclipse all others as the seat of original research, would, if I understand the views of the donor, fail utterly to carry out his intention. The attitude of the Carnegie Institution, if I understand the thought of the founder, is to be that of the gracious handmaid of learning, intelligently ministering to those who need, and without such agency could not have, help.

As everybody knows, the donor of this fund had in his mind not so much institutions as individuals. His thought could not be more felicitously expressed than he has himself expressed it in the words which you quote, in which he states the main object of his foundation to be 'to discover the exceptional man in every department of study whenever and wherever found, inside or outside of schools, and enable him to make the work for which he seems specially designed his life work.' Mr. Carnegie's large knowledge of men has taught him that there are 'exceptional men,'—very often men poor in purse, but rich in enthusiasm and in mental power, who need but the helping hand to enable them to achieve great things, not only for themselves, but for mankind, and nowhere are such exceptional men more numerous than in the ranks of the scientific investigators of this country. They would not be scientific investigators were they not possessed of power and filled with the love of truth. Even as I write I can think of a score of such men, who are struggling in the midst of adversity and prevented by the *res angusta domi* from achieving tasks the doing of which would bring luster upon their names and honor to the nation whose sons they are. Such men deserve to be helped, and in helping such men the Carnegie Institution will place the highest crown of glory upon its head. To hold the Institution more or less rigorously to this phase of activity seems to me to be the plain duty of those who are charged with its administration.

A few quite practical and concrete suggestions based upon personal experience as to the manner in which this fund might be utilized to promote the advancement of science in America in cooperation with existing institutions may not be wholly out of place. I am emboldened to throw out these suggestions by your example, seeing that you have appealed to your own experience in your own line of special research.

In order to enable scientific men to work rapidly and successfully to their ends, especially in the field of the biological sciences, it is of prime importance to them to have access to collections which embody in themselves the results of the investigations of those who have gone before them. This is particularly true in mineralogy, botany, and zoology in its various branches. When a student has devoted himself to the study of one of these branches of science and has, by years of

labor and effort, amassed collections which are determined with absolute scientific correctness, and which contain the 'types' upon which he has founded his published descriptions, these collections become at once classic as a court of last appeal in all cases of doubt. The retention of such collections, especially when they relate to the mineralogy, botany and zoology of a country, within easy access of the students of that country, is a matter of incalculable importance. American science has suffered severely in past years because of the failure of American institutions, often because of lack of money, to keep within the limits of the United States scientific collections, reference to which on the part of the student is necessary. The most eminent student of certain groups in entomology in this country a number of years ago was Alexander R. Grote, to-day connected with the Roemer Museum at Hildesheim. Mr. Grote was the first man who began systematically to study the moths of America and to name and describe them. His 'types' were contained in his collection for the most part. Pressed by financial necessities, Mr. Grote sold this collection to the trustees of the British Museum. The consequence of this fact is that to-day pilgrimages are annually performed across the Atlantic Ocean at considerable expense of time and money by American students in order to consult this classic collection. The trustees of the British Museum, I believe, paid something like three thousand dollars for the collection. I am aware that American students of entomology have already spent out of their private purses many times this amount in traveling across the seas to consult it. A few years ago the great collection of William H. Edwards was on the point of going in the same way. Personally I determined to save it for the students of America, and I purchased it myself, and it is to-day accessible in the Carnegie Museum. One function of the Carnegie Institution, it seems to me, might well be to aid the great reference museums of America to retain within easy reach of our scientific workers collections of this character, the loss of which to the land is practically irreparable. As a rule such collections are not vastly expensive, but their loss to the American student is a positive calamity, and I trust that the trustees of the Carnegie Institution will make it a point to cooperate with the heads of our great museums in preserving for the students of American science the types of all American species. Nothing more positively beneficial in the direction of the advancement of science could be done than this, as I am sure all botanists and zoologists will agree with me in unanimously declaring.

Finally, I wish to assert my unqualified subscription to your statement that the Carnegie Institution should do only that which will not conflict with existing institutions, but aid them, and secondly, should aim to improve the condition of men of science, working with them and through them. The only men in this connection whom we have to fear are, I think, the class whom I am pleased to call the 'political scientists,' the men who look upon scientific positions as 'jobs.' There are a few such men, to the honor of science be it said not many.

W. J. Holland.

Carnegie Museum, Pittsburgh,
September 23, 1902.

To the Editor of Science: It may as well be conceded first as last that the Carnegie Institution will have a definite location. However much any of us might wish to see the experiment made of a great institution managed from an obscure little office on a side street, it is extremely unlikely that any such thing will be done. The Institution must have office and other

rooms for the transaction of its business. These should be commodious, and adapted to the needs of the officers, and the building as a whole should have a dignity commensurate with the rank of the Institution. In the second place, I venture to say that the Institution must be a *unit*. Neither its founder nor its managers are likely to consent to a policy which will result in such subdivision of the income as will fritter it away in many ineffectual driblets. There is not enough money to endow research along many lines. It is impossible to endow scientific journals, support marine and other laboratories, aid considerable numbers of worthy individuals, and conduct original investigations along several lines. Many people have been dazzled with the size of the principal, and talk as if the ten millions of dollars *were available annually*, forgetting that it is only the income from this sum which is available. This income, after all, is not so very large. Already many of the universities of this country greatly exceed it.

Evidently the work undertaken must be definitely limited. It must be concentrated upon certain phases of investigation and instruction, and in this way it may hope to aid the progress of human knowledge. It seems to me that many of the suggestions as to the policy to be pursued by the trustees of the Carnegie Institution fail in that they appear to be based on the supposition that it is to be over and above all existing ones—a sort of supreme educational establishment of the university type. Yet it can be no such thing. Had the fund been ten times ten million dollars, the Carnegie Institution might have overtopped Harvard, Yale, Columbia, Chicago, Stanford and all the rest of the universities of the country. But it is idle to think of any such thing with the income which the present fund will yield.

What, then, can be done? Clearly the trustees should avoid duplicating what is already fully provided for in existing institutions. In the institution which they establish they should contribute something to education and educational thought. The Smithsonian Institution taught us the value of original research, and its 'Contributions to Knowledge' will stand for all time as evidence of the high standard set by it. Johns Hopkins University has made one contribution of the greatest importance —namely, graduate study in the American University. It may be said to have fixed the standard of graduate work, and every educational institution in this country has been helped by its example.

Now let the Carnegie Institution set for itself one good piece of work, and concentrate upon that, rather than fritter away its income in many little benefactions, all more or less worthy and commendable, but already under the care of some other institution.

I suggest, therefore, that the trustees found an 'Institute,' which shall carry the work in some rather narrow department of knowledge far beyond the boundaries possible to be reached by university departments. It is impossible to support a great university by the income from this bequest, but it is possible to maintain an 'institute' devoted to some branch of investigation. This might be a chemical institute, a physical institute, a zoological institute, a botanical institute, a geological institute, a physiological institute, a pathological institute, a psychological institute, etc. I cannot decide which of these should be inaugurated; that may well be left to the trustees and the president of the institution. Let me suppose (since I am not a chemist, and therefore am not pleading for my own subject) that the decision is to found the 'Carnegie Chemical Institute'; we might then hope to have in it the best facilities known for the solution of chemical problems. Here

might be brought as professors some of the foremost masters of the subject in general, as well as many specialists in particular fields of the science. Here might be admitted as students such men as have made marked progress in advanced lines of work in the better universities, and who are prepared to continue work in the institute. I should not favor free tuition, nor the establishment of stipend-bearing fellowships. On the contrary, I should favor the policy pursued at Johns Hopkins University of making the usual charge for tuition. Men who are prepared to continue work in the institute always will be able to pay the usual academic fees. Fellowships carrying stipends would no doubt attract students, but it is not numbers which the institute wants, as much as students of the highest ability—and such rarely, if ever, need to be induced to continue work by the promise of a stipend.

With one well-endowed institute in Washington on the Carnegie foundation, we might hope that ultimately the several sciences would be similarly provided through the benefactions of liberal-minded men of wealth. These institutes would then be for the present the highest development of the educational facilities of the country, in these lines, and the successive steps in the system would be as follows: Primary schools, secondary schools (high schools and academies), colleges, universities, institutes. In these there is a constantly decreasing number of students, who proceed in their educational development from the general to the special—from the universal to the limited. It is for the limited number of specialists who have come up through all the preceding steps that the institutes should exist. I suggest, therefore, that the trustees inaugurate an institute of this highest grade.

CHARLES E. BESSEY.
THE UNIVERSITY OF NEBRASKA.

THERE is undoubtedly a great work to be done in starting local investigators through correspondence. I have always, during a number of years, had several such men on my list—have hunted references in works they did not possess, gone over their MSS., suggested lines of investigation, and so forth. The result has on the whole been most gratifying. These men have not always been isolated in the ordinary sense of the word; not rarely they have been graduate students in our best colleges and universities. I need hardly say that I have received much help of the same kind. I believe that any man who is familiar with a particular branch of study can do this sort of helping work, and that it is extremely worth while. But of course it brings no pay, and it could, I think, very well be subsidized in some way.

There are many good investigators scattered about the country, who don't accomplish anything for lack of help and kindly criticism. Often the mere fact of not having some expensive work seems to put a stop to an investigation. But the specialist of long standing can look up references and take away this difficulty. To merely offer the beginner money would not meet the case at all; he needs guidance.

Please understand that I *don't* propose a plan whereby young men may have their work done for them. Directly they show a desire to build their 'researches' out of other people's brains they should be dropped. But this does not apply to those who are really doing all they *can* and are hindered by circumstances beyond their control. Frequently the circumstances are such that the work can only be brought to a fruitful state through a good deal of cooperation; then the published results should indicate the fact, and the two or more names appear on the title page.

I think some special regard should be had for the thinly settled parts of the

country. In these regions we find, (1) that the scientific men are extremely few, (2) the means for their support are still fewer, (3) and that there is a superabundance of opportunities for study. In New Mexico I am the only zoologist, so far as I know (unless the paleontologists Springer and St. John are regarded as zoologists*). There is no support of pure science anywhere in the territory. Yet the opportunities for research are innumerable. (Of course they are so anywhere, but there are so many almost or quite virgin fields in New Mexico; so many whole groups of animals unstudied, whole mountain ranges unexplored by the biologist.)

My ideal is to have the means to invite a dozen or more young men (or women) out here to take up some of the lines of work I see open in every direction. I try to do what I can, but the things I *can't* do are never out of my mind.

I think your remarks on the subject of publication are very wise. This is a subject of the first importance. In zoology and botany great good could be done by publishing catalogues, bibliographies, etc. For example, I understood that Mr. S. Henshaw, of the Mus. of Comp. Zool., has in MSS. a work giving references to the whole of the literature on North American Coleoptera, with localities. I understand that he cannot find any one to publish it. It would be simply invaluable to the student of geographical distribution and to the coleopterist.

Faunal works also deserve support. I believe Dr. J. B. Smith, of New Jersey, stands ready to publish a work on the noctuid moths of North America, if any one will relieve him of the cost of printing.

I hope, however, that the Carnegie Institution will give us most of its publications at reasonable prices and with as little paper as may be needful for good printing. The bulky quartos so often published are quite too costly and too heavy to carry about.

These remarks are of course only meant to cover a small amount of ground, in which I happen to be interested. I have no disposition at present to discuss the Carnegie Institution as a whole.

Theo. D. A. Cockerell.

* C. L. Herrick has formerly published on zoology, but is not now working on this branch.

The problem before the trustees of the Carnegie Institution is not simply that of the profitable administration of the fund in the promotion of research—this would be easy enough; but it is to secure the greatest possible enlargement of the bounds of human knowledge from an income, which, large though it seems, is but small in comparison with the amount being spent upon research the world over. Now there is but one well-spring of new knowledge, and that lies in certain rare individual minds with an inborn aptitude, needing to be supplemented by a special training and a favorable environment, for scientific research. Knowledge is advanced in depth, if not in breadth, far more by the single occasional genius than by many lesser minds. To find out, and especially to give full play, to these few rare minds, seems to me the true ideal of the Carnegie Institution, and the object to which the greater part of its fund may most profitably be devoted. There are two ways, practically, along which to work towards this end. First, wherever there is known to be a man who had proven a marked capacity for research, but who has been forced by circumstances into an unfavorable environment, he should be offered a stipend, not lavish, but ample for the support of himself and family in ordinary comfort, to enable him to remove for a year or two to any center of research he may choose; if then his work goes well, he should be granted a second

year, and a third, and, finally, if it seem profitable, even a lifetime. Second, the Carnegie Institution should take up trained and promising young investigators where our universities leave them. The university is the natural and efficient, though by no means the exclusive, selection and training ground for investigators; it is in the ability to permit these men to continue their investigations that our American universities are weak, and need to be supplemented. A few of the best of these young men, the ones most highly recommended by the faculties of the leading universities, should be offered stipends large enough to permit them to live in comfort at any center of research they may select, for a year, or for two, or for three, or for a lifetime, according as their results show to be profitable. From the many called to a year or two of such honorable activity few would be chosen for a lifetime, but those few would form a priceless possession to humanity.

To provide a favorable environment for minds adapted to research seems to me, therefore, the best use for the greater part of the Carnegie fund. But, second to this, there are certain other profitable uses for a part of it—the purchase or construction of apparatus for use in promising investigations by private investigators, grants to scientific expeditions or in aid of bibliographies, subsidies to investigating laboratories and to scientific publications, and many minor worthy objects of like sort.

There are two uses to which I think none of the funds of the Carnegie Institution should be put. First, they should not be used to duplicate any existing institutions for research, and especially not for the erection or purchase of laboratories of any kind in Washington or elsewhere. In this country, it seems to me, our material facilities for scientific research already far exceed our capacity to utilize them profitably. In botany, for example, the development of institutions is out of all proportion to the importance of the results which are coming from them. The Missouri (Shaw) Botanical Garden, the New York Botanical Garden, and in lesser degree several other institutions, are offering freely to all investigators facilities for botanical investigation which money can hardly improve upon; what is now wanted is not more such institutions, but more men capable of making proper use of them. I cite botany because I know it better than the other sciences, but I presume the same is true in this country of most, if not all, of the other sciences. To duplicate facilities not already fully utilized would be most wasteful. There is, moreover, another reason why I think the Carnegie Institution should not own any laboratories of its own, including such an one as that at Woods Hole, namely, the temptation to aggrandize those particular laboratories would be so great, and the capacity of any laboratory in the endlessly expanding sciences to absorb money is so nearly boundless, that all of the fund available for each particular science would in time, if not soon, be absorbed to that particular use, and other objects, however worthy, would be no better off than at present. Second, the funds should not be used for any form of gratuities, rewards or prizes, or to pay to investigators salaries or stipends larger than needful for comfortable living and the successful prosecution of their researches. Prizes have their uses in the lower grades of intellectual activity, but to suppose that pure scientific research of the highest type is appreciably promoted by them seems to me to involve an erroneous idea of the mental attitude of the investigator towards his results. At all events the utility of such rewards is, on the one hand, not demonstrated by the history of scientific progress, while, on the other, the efficiency of prizes is being ex-

perimentally tested on a gigantic scale by the Nobel bequest, and the Carnegie Institution can well afford to await the results. I take it the chief reward of the genuine investigator consists in the accomplishment of the work itself, in the moments of exhilaration when truth new to the race first dawns upon him, in the approbation of his peers. If he does not do the best there is in him for these, he will not do it for the trappings which a salary larger than needful for comfortable living will enable him to buy.

The wisdom of devoting the most of the funds of the Carnegie Institution to the selection and cultivation of individual investigators seems to me the more important in view of the fact that Americans appear to be weak in the investigating instinct, or temperament. The genius of the American people is rather for affairs than for that patient persistent microscopic application which is the soul of research. It is all the more needful, then, to seek out and cultivate such investigating talent as there may be. To suppose that it is money alone that is now needed to give this country the primacy in research is to share the attitude of the man who, become suddenly rich, said to his son, 'My son, we are now very rich and you can realize your ambition to become an author; yes, we are rich enough so that if you wish you can become a great author.' It will call for much from the Carnegie Institution besides its great income to make this country great in profound scientific research.

I think, therefore, that the highest usefulness of the Carnegie Institution will lie in acting as a special providence to men, institutions and events, concerned in the advancement of human knowledge. As such it must be content with the rewards of the spirit, and willing to forego structures and furnishings visible to the physical eye, which in this case should be so much the easier for the reason that the munificent founder of the Institution is already amply honored in the many sightly and serviceable structures with which the land so happily abounds. W. F. Ganong.

Smith College,
Northampton, Mass.

I suppose that every scientific man, who has at any time been hampered in his work by lack of funds—as which of us has not?—allowed himself, when he heard of Mr. Carnegie's millions, to dream of what could be done, with unlimited money, for his own science. My own thoughts turned at once to the building and equipment of adequate laboratories of experimental psychology. For we psychologists have no laboratories that can at all compare with those of physics or chemistry or biology, or that at all worthily represent the range and complexity of our science. The student of physics, at any one of the larger institutions, is impressed as he enters the laboratory with the dignity and importance of the work before him; physics is largely housed and richly equipped. It is very different with psychology. An old building that has outlived its original usefulness, a private house that the university does not need, a set of rooms in the corner of some building devoted to miscellaneous purposes—these are our laboratories. No museum rooms for the display of historical instruments; no private laboratories for the instructing staff; no proper separation of teaching and investigation. What I should most of all like to see, then, is a special laboratory building, specially designed for psychological ends, adequately officered, and ample enough to accommodate all the many branches of psychological work. It would not much matter where the building was placed, provided that it existed, and were reasonably accessible. Once a model was made, improvement would follow all round.

I realize, however, that psychology has more immediate and pressing needs, that can also be more easily satisfied. First among these I should place the need of help in publication. There can be no question, as Professor Cattell has said (SCIENCE, September 19, 464), that the present difficulties in the way of publication are lamentable. Every year we have, in my own laboratory, to make some sacrifice to the cost of printing: dropping out an historical chapter here, cutting out tables there, and what not. We all know, of course, that the doctorate thesis is likely to be spun out to an unnecessary length; and I am not sure that the fulness of detail affected by certain of the continental journals of psychology is not a distinct hindrance to the science. But it is an indisputable fact that, in America, really good work, work that has been condensed to its limit and that ought to be published, is time and time again held back from the printer because the author or the journal is too poor to print it. Hence I heartily endorse all that Professor Cattell has said under this heading.

In the second place I should put the need of scholarships and fellowships, and of subsidies to students and professors. There is much to be said for and against our present system of graduate scholarships. One thing must, however, be borne in mind: that the appointment of a man, in his last undergraduate year, to a graduate scholarship always carries with it something of a risk. Undergraduate promise is not always fulfilled, and testimonials are slippery things. So that the number of scholarships available for a particular science should be large enough to allow of a good percentage of failures. Failures, I mean, from the point of view of the science; for any man of decent intelligence must be helped towards his life-work by a year of graduate study, whether he continue it further or not. If the science is ultimately to get a fair share of good men, it must have a large number of students to select from. I should, therefore, see no harm, but rather good, in an increased number of graduate scholarships and fellowships. But I regard two possible modifications of the existing system as more important than its mere enlargement. On the one hand, we need at each university a few really valuable fellowships, say of $750 or $1,000 for two or three years; endowments that should allow the exceptional man to do an elaborate piece of investigation before he enters on his teaching career. And on the other, we need, I think, a certain fund for subsidies that should not be looked upon as university honors, but should simply give opportunity of graduate work to men who are too poor to undertake it on their own account and yet too promising to be let slip: subsidies of, perhaps, $300 or $400 for one year. I believe that both of these forms of endowment are sorely needed by psychology,—and one can speak primarily only for one's own science; and I believe that they would do much more good than the establishment of additional scholarships on the present basis.

I have put the student before the professor. I regard, however, the helping of the professor by occasional subsidy as of equal importance with the helping of the graduate student. My colleagues will bear me out that there are often times when a gift of $500 or $1,000 would ensure the accomplishment of a bit of personal work for which one is reluctant to draw upon the general fund of the laboratory, even if the general fund would stand the drain. There has been some discussion in SCIENCE of the reason for lack of appeal to existing research funds. There are two obvious reasons. The one is that the professor, with the pressure of teaching and of routine departmental work upon him, cannot as a rule see his way clear enough ahead (say, for two or three years) to justify his asking for a

definite sum for a definite purpose; and the other is that though one may see where work needs to be done, where there is a promising opening, one cannot (I speak again for psychology) guarantee results or even formulate one's program until the investigation is well started. Nevertheless, scientific moneys can hardly be placed in better hands than in those of men whose lives are devoted to science, and who have proved their competence by their own work and by that of their pupils.

In summary, then, I should advocate: (I.) increased facilities of scientific publication, and (II.) scientific endowments of three kinds. These are (1) the establishment of a few valuable fellowships; (2) the granting of a living wage for one year to men of promise; and (3) the unhampered gift of sums of money to men of scientific eminence —passed upon, perhaps, by a committee of their peers—on their personal guarantee to do with the gift what it lies in their power to do for the advancement of science.

E. B. Titchener.

Cornell University.

In response to the general invitation and a special request from the editor of Science, it is a pleasure to suggest two or three lines of policy which seem worthy of consideration.

It may be premised that the suggestions grow out of the express intent of the founder to promote science by affording opportunities for men; and it may be noted in passing that this intent is so far distinctive as to permit the development of an institution occupying an essentially unique plane: It is the function of the university to mold men according to the image; it is the function of the official bureau to have ready-molded men mold and apply knowledge according to accepted standards; but it would seem to be the Carnegie idea to permit and help men to mold both themselves and knowledge in the light of their own genius as well as in that of current experience—an idea precisely in line with the course of human development as seen by the anthropologist. In conformity with this idea, it would seem clear that the new establishment should scrupulously avoid fields already occupied by universities and colleges on the one hand, and by federal and state bureaus of scientific character on the other hand; and it would seem to follow, as already pointed out by Professor Cattell, that the new institution should dispense with plant and other material encumbrances to the fullest possible extent. The suggestions are made in accordance with this view.

1. The first suggestion (which is but a repetition of one made by Professor Cattell) is that the purposes of the founder be carried out largely through the creation of fellowships in special lines of research. It may be added that the lines of work should be adapted to the ambitions and capabilities of particular candidates or nominees for fellowship, and that novel lines of inquiry should be tolerated no less kindly than the conventional lines pursued in the purely educational institutions. The fellowships might be either fixed or variable, or might be graded, *e. g.*, at $600, $1,000 and $1,500; but in any event the financial measure should be determined by the primary object of the Institution, *i. e.*, that of giving the *man* an opportunity of pursuing knowledge. The fellowships might properly continue over two, three or five years, but should not be regarded as permanent.

2. The second suggestion is that every fellow should be allowed and expected to gain distinctive permanent recognition for excellent work in his special line, in the form of some honorary degree or designation. A single order (which might be styled master) might suffice; while the classes

should be special, unlike those conferred by institutions of learning, and determined by special work. Thus, there might be masterates of agriculture, of paleontology, of terrestrial physics, of mineralogy, of entomology, of ethnology, etc., but not of arts, or philosophy, or laws, or science. The degrees should be special, the well-earned reward for special work; they should be credentials rather than titles; and the number of classes should be unlimited, in conformity with the modern multiplication of specialties as well as the fundamental idea of developing individuality—of making men rather than schoolmen. The masterates should, of course, be permanent, and should not involve financial relations with the Institution—*i. e.*, masters should be neither entitled to, nor debarred from, support by the Institution.

The terms 'fellow' and 'master' are not without objection, chiefly on the score of current use in other connections; they merely serve the purpose of these suggestions. The two classes would correspond roughly with the apprentices (or perhaps rather the journeymen) and masters of an important stage in industrial progress; they would seem to bridge and unite the two great buttresses of human advancement, *i. e.*, the intellectual development of the schools and the manual development of the shops.

The advantages of the masterates would be twofold: In the first place, they would afford incentive and stimulus to hard-working fellows; in the second place, they would form a permanent bond between the Institution and its beneficiaries and among the beneficiaries themselves, producing an *esprit de corps* by which the usefulness of the Institution would be most effectively extended and perpetuated.

3. The third suggestion is partly an extension of the second, partly the outcome of current needs. Among the means of promoting science in this and other countries, conferences among scientific men take a high if not the leading rank; and the demand for such meetings has been met by the creation of a large number of voluntary organizations. In this country, at present, there is a tendency to form special societies of national character (such as the American Chemical Society, the Geological Society of America and the American Anthropological Association), and more general societies or academies of largely local membership; and the effect is to increase the need for such more general organization among scientific societies as will lead to better coordination of effort among scientific workers. It has already been pointed out that this great and growing need would be met by a general delegate organization which might be called a Senate of Science (SCIENCE, Vol. XIV., pp. 277–280), and it was also pointed out that the chief obstacle in the way of organization of such a body would be the cost of the requisite journeys by delegates. Now it would seem appropriate for the Carnegie Institution to become a nucleus for such a general scientific organization, to be made up of delegates chosen for fixed terms by the scientific societies of the country, and to be maintained for the purpose of fostering and encouraging scientific activity; and that a fraction of the current funds available through the munificence of the founder be so expended as to place all delegates on an equal footing by the payment of necessary traveling expenses to the points selected for the meetings. Such an arrangement would undoubtedly kindle the interest and sympathy of scientists and scientific associations generally, and, like the establishment of masterates, serve to extend and perpetuate the influence of the Institution.

The foregoing suggestions of course imply the creation and maintenance of an executive mechanism with the least prac-

ticable expenditure for material or administrative purposes, and with the idea of allowing the light of a noble institution to shine afar, to enter the darkest corners of the land, to stir dormant genius everywhere, to awaken every germ of scientific activity.

W J McGEE.

PROFESSIONAL SCHOOLS AND THE LENGTH OF THE COLLEGE COURSE.*

STANDARD OF ADMISSION TO THE PROFESSIONAL AND TECHNICAL SCHOOLS.

I HAVE pointed out that it is held to be settled policy at Columbia University that the several technical and professional schools shall rest upon a college course of liberal study as a foundation (although not necessarily upon a course four years in length), either at once or as soon as practicable. The School of Law has already been placed upon the basis of a graduate school, to take effect July 1, 1903. On December 20, 1898, the University Council recommended that the College of Physicians and Surgeons be made a graduate school as soon as such a step is financially practicable. The Schools of Applied Science have constantly in mind a similar step, and much consideration has been given by the faculty to the best way of bringing about the change without undue sacrifice. This policy, however, does not pass unchallenged. It has recently been criticised and opposed in a cogent and noteworthy argument by President Hadley, of Yale University, in his annual report for the year 1901–02, on the grounds (1) that it tends to make the professions exclusive in a bad sense, (2) that it leads to a remodeling of the college course to meet the needs of intending professional students, which remodeling is at least a doubtful experiment, and (3) that it establishes an unfortunate distinction between the universities which require a bachelor's degree as a condition of admission to the professional schools and those which make no such requirement. This policy is also criticised and opposed by many intelligent persons, trusted leaders of public opinion, not university teachers or administrators, who are impressed by the fact that the whole tendency of our modern educational system is to prolong unduly the period of preparation or studentship, with the result that an increasing number of young men are held back from active and independent participation in the practical work of life until they are nearly, or quite, thirty years of age. In the face of such objections as these it is obvious that we at Columbia must consider carefully the probable social and educational effects of the policy upon which we have entered.

* From advanced sheets of the annual report of President Butler to the trustees of Columbia University.

The questions raised in the discussion of this policy are to be decided, it seems to me, from the standpoint of the duty of the university to the public and to its own educational ideals. Two interests are immediately at stake: the standards of professional study in a university, and the place of the American college in the higher education of the twentieth century. I doubt whether the two interests can be separated in any adequate consideration of the subject.

President Eliot, of Harvard University, impressively set forth the responsibilities and the opportunities of the learned professions in his address at the installation ceremonies on April 19 last, when he said:

> It is plain that the future prosperity and progress of modern communities is hereafter going to depend much more than ever before on the large groups of highly trained men which constitute what are called the professions. The social and industrial powers, and the moral influences which strengthen and uplift modern society are no longer in the hands of legislatures, or polit-

ical parties, or public men. All these political agencies are becoming secondary and subordinate influences. They neither originate nor lead; they sometimes regulate and set bounds, and often impede. The real incentives and motive powers which impel society forward and upward spring from those bodies of well-trained, alert, and progressive men known as the professions. They give effect to the discoveries or imaginings of genius. All the large businesses and new enterprises depend for their success on the advice and cooperation of the professions.

With such an ideal as this held up before the student of law, of medicine, of divinity, of teaching, of architecture or of applied science, what standard of excellence shall the university require of him when he enters upon his professional studies? Three answers seem to be possible: The university may require (1) the completion of a normal secondary school course of four years, and so put admission to the professional and technical schools on a plane with admission to college, or (2) the completion of the present college course of four years, or (3) the completion of a shortened college course.

When weighing the advantages and disadvantages of these several lines of action, it should be borne in mind that a uniform policy on the part of all universities in dealing with this question is not necessary and may not be desirable. We are directly concerned with the question so far as it concerns the duty and the interest of Columbia; but the universities having different social and educational needs to meet, and somewhat different ideals to labor for, may be wise in reaching a conclusion quite different from that which most commends itself to us. This consideration seems to me to meet the third of President Hadley's objections already referred to. Furthermore, the universities do not control admission to the practice of the professions, and it is not in their power, as it is certainly not their wish, to shut out from his chosen profession any competent person, whatever his training or wherever it has been had. If the standards of professional study required by the universities are higher than the minimum fixed by law, no one will attend a university for professional study unless its standards appeal to him and unless he hopes to find ultimate gain by conforming to them at some expense of both time and money. On the other hand, if the universities make the minimum standards fixed by law their own—and only by so doing can they avoid discriminating against some one—then they seem to me to have abdicated their functions as leaders in American intellectual life. The result would quickly be seen, I am sure, in the falling off of popular favor and support. These facts appear to meet the first of President Hadley's objections. His second objection involves a discussion of the significance of the college course, a subject which I shall consider in its proper place.

Columbia University cannot be satisfied with a requirement of only secondary school graduation for admission to the professional and technical schools for several reasons.

1. Such students at 17 or 18 years of age (or, as should be the case, at 16 or 16½ years) are too immature to carry on a severe course of professional study with profit.

2. When such students predominate, or form a large proportion of the total number attending any given professional school, the teaching deteriorates and the instruction tends to become either superficial or unduly long drawn out and wasteful of time.

3. Other institutions in various parts of the country afford the fullest opportunity for students who are compelled to remain satisfied with the shortest possible preparation for the practice of a profession, and Columbia would not be justified in using its funds merely to add to a provision

which is already ample. Columbia offers the most generous assistance to students who are able and willing to meet its standards and who need help in order to carry on their studies, but is not willing to lower those standards at the cost of social and educational effectiveness.

4. Secondary school graduates, however well taught, are necessarily without the more advanced discipline in the study of the liberal arts and sciences and without that wider outlook on the world of nature and of man which it is the aim of the college to give. It is our hope and wish that those who hold professional or technical degrees from Columbia University will be not only soundly trained in their chosen professions, but liberally educated men as well. No stress is laid upon the college degree as a mere title, but it is held to stand, in the vast majority of cases, for greater maturity of mind and broader scholarship.

5. For Columbia University to admit students to the professional and technical schools upon the same terms as those by which admission to the college is gained, would be to throw the weight of our influence against college education in general and against Columbia College in particular. After a few years, no student who looked forward to a professional career would seek admission to Columbia College, or to any other, except those who had ample time and money to spare.

On the other hand, while I hold a secondary school education to be too low a standard for admission to professional study at Columbia University, personally I am of opinion that to insist upon graduation from the usual four years' college course is too high a standard (measured in terms of time) to insist upon, and an unsatisfactory one as well. My view of the matter is concurred in by the dean of Columbia College, by the dean of the Faculty of Law, and by the dean of Teachers College, as will be seen by reference to their annual reports, which accompany this document and are a part of it.

My objections to making graduation from a four years' college course a prerequisite for professional study at Columbia University are mainly two:

1. I share the view, already alluded to, that the whole tendency of our present educational system is to postpone unduly the period of self-support, and I feel certain that public opinion will not long sustain a scheme of formal training which in its completeness includes a kindergarten course of two or three years, an elementary school course of eight years, a secondary school course of four years, a college course of four years, and a professional or technical school course of three or four years, followed by a period of apprenticeship on small wages or on no wages at all.

2. Four years is, in my opinion, too long a time to devote to the college course as now constituted, especially for students who are to remain in university residence as technical or professional students. President Patton, of Princeton University, voiced the sentiments of many of the most experienced observers of educational tendencies when he said that: "In some way that delightful period of comradeship, amusement, desultory reading, and choice of incongruous courses of what we are pleased to call study, which is characteristic of so many undergraduates, must be shortened in order that more time may be given to the strenuous life of professional equipment." For quite twenty years President Eliot has advocated this view and in arguments which have seemed to me unanswerable, under the conditions existing at Harvard, has urged that the degree of bachelor of arts be given by Harvard

College after three years of residence.* At Columbia, and elsewhere, the practice of counting a year of professional study as a substitute for the fourth or senior year of the college course has in effect established a three years' college course for intending professional and technical students. The degree has been withheld until a year of professional study has been completed, in deference to tradition rather than from sound educational principle. In this way new conditions have been met without the appearance of shortening the college course. While the policy hitherto pursued in this regard was justified as a beginning toward a readjustment of the relations between the college and the professional and technical schools, it is hardly to be upheld as a final solution of the problems presented. From my point of view it is open to criticism in that it (1) shortens the college course without appearing to do so, (2) divides the interest of the student in a way that is satisfactory neither to the college nor to the faculties of the professional schools, and (3) fails to give the full support to a college course of purely liberal study which is so much to be desired.

There remains a third line of action, namely, that of basing admission to the professional and technical schools of the university upon a shortened course in Columbia College or its equivalent elsewhere. This I believe to be the wisest plan for Columbia University to adopt, as well as the one whose general adoption would result in the greatest public advantage.

* After this report was in type it was announced that hereafter the degree of A.B. will be conferred by Harvard College upon students who complete the requirements for the degree in three years at once and without an additional year's delay, as heretofore. Somewhat similar announcements have also been made by the University of Pennsylvania and by Brown University.

LENGTH OF THE COLLEGE COURSE.

One consideration of vital importance appears to have been overlooked in the numerous discussions of this whole matter, and that is the fact that there is no valid reason why the college course should be of one uniform length for all classes of students. The unnecessary assumption of the contrary view has greatly complicated the entire question, both in the public and in the academic mind. It must be remembered that for the intending student of law, medicine or applied science who goes to college, three or four additional years of university residence and study are in prospect after the bachelor's degree has been obtained. For the college student who looks forward to a business career, on the other hand, academic residence closes with graduation from college. For the latter class, therefore, the college course may well be longer than for the former. While two or three years of purely college life and study may be ample for the man who proposes to remain in the university as a professional or as a technical student, three, or even four, years may be desirable for him who at college graduation leaves the university, its atmosphere, its opportunities, and its influence, forever.

It must be remembered, too, that the four years' college course is merely a matter of convention, and that there are many exceptions to the rule. The Harvard College course was at one time but three years in length, and the collegiate course at the Johns Hopkins University has been three years in length from its establishment. The normal period of residence for an undergraduate at both the English and the Scottish universities is three years. President Wayland, of Brown University, who was in so many ways a true prophet of educational advance, devised a plan for a normal three years' college course over half

a century ago. The question is not so much one of the time spent upon a college course as it is one of the quality of the work done and the soundness of the mental and moral training given. The peculiar service which the college exists to perform may be done in one case in two years, in another in three, in another in four, and in still another not at all.

Since 1860 the changes in American educational conditions have been revolutionary, and as one result the content of the A.B. degree has been wholly altered and that degree has been elevated, at Columbia College at least, to a point almost exactly two years in advance of that at which it then was. In other words, despite the fact that college admission requirements have been raised and much of the instruction once given in college is now given in the secondary schools, particularly the public high schools, the bachelor's degree has been held steadily at a point four years distant from college entrance, with the result that the average age of college students at graduation has greatly increased. Since 1880 the average age of the students entering Columbia College has increased exactly one year, and while no adequate statistics for 1860 are available, it appears to be true that the average age of admission in 1880 was one full year higher than in 1860. The registrar has made a careful examination of the official records, and reports that in Columbia College we are demanding two years more of time and work for the degree of bachelor of arts than was required in 1860, and one year more of time and work than was required in 1880. President Hyde, of Bowdoin College, has recently said that 'Nearly all the distinguished alumni of Bowdoin College graduated at about the present average age of entrance, and were well launched on their professional careers at about the age at which our students now graduate.' He cited the cases of Jacob Abbott and William Pitt Fessenden, who were graduated before they were seventeen; Longfellow, who was graduated at eighteen; Franklin Pierce, John A. Andrew, Fordyce Barker, and Egbert Smyth at nineteen; and William P. Frye and Melville W. Fuller at twenty. Instances might readily be multiplied from the records of the American colleges. The recent statistics compiled by Dean Wright, of the academical department of Yale University, which show the average age of graduation of the members of the class of 1863 at Yale to have been 22 years, 10 months, and 17 days and that of the members of the class of 1902 to have been 22 years, 10 months, and 20 days, point to what appears to be a striking exception, not yet explained, to the general rule.

So long as there were no graduate schools, and therefore no genuine universities, in the United States, and when the bachelor's degree was the highest academic distinction to be gained in residence, it was sound academic and public policy to make the requirements for the degree of bachelor of arts as high as possible. It was the only mark of scholarship that the colleges could give. As a result, the average age at graduation increased. Now, however, conditions have entirely changed. Nearly, or quite one half of the work formerly done in college for the degree of bachelor of arts is now done in the rapidly increasing number of secondary schools, particularly public high schools, and no small part of it is required for admission to college. This does not appear if the comparison be restricted to admission requirements in Greek, Latin and mathematics; but it is clearly evident when the present admission requirements in English, history, the modern European languages and the natural sciences are taken into account. The standard of scholarship in this country is no longer set by the undergraduate courses in

the colleges or by the time devoted to them, but by the post-graduate instruction in the universities and by the requirements demanded for the degree of doctor of philosophy.

These being the undisputed facts, it would appear to be wise, and possible, to treat the length of the college course and the requirements, both in time and in accomplishment, for the degree of bachelor of arts from the standpoint of present-day needs and the largest social service.

In my opinion it is already too late to meet the situation by shortening the college course for all students to three years, although such action would be a decided step forward so far as the interests of intending professional and technical students are concerned. When President Eliot first proposed a three years' course for Harvard College, the suggestion was, I think, a wise one. But in the interval conditions have changed again. If we at Columbia should be willing to go no farther than to reduce the length of the college course from four years to three, we should (1) find it impracticable both on financial and on educational grounds to require that course as prerequisite for admission to the Schools of Applied Science, and, possibly, to the School of Medicine, and (2) we should be unable to resist the pressure for further reconstruction and rearrangement that would be upon us before our work was completed and in operation. My own belief is that Columbia University will perform the greatest public service if it establishes two courses in Columbia College, one of two years and one of four years—the former to be included in the latter—and if it requires the satisfactory completion of the shorter course, or its equivalent elsewhere, for admission to the professional and technical schools of the university. By taking this step we should retain the college with its two years of liberal studies as an integral element in our system, shorten by two years the combined periods of secondary school, college, and professional school instruction, and yet enforce a standard of admission to our professional schools which, both in quantity and in quality, is on a plane as high as the Columbia degree of bachelor of arts of 1860, which was recognized as conforming to a very useful standard of excellence. At the same time we should retain the four years' course with all its manifest advantages and opportunities for those who look forward to a scholarly career, and for as many of those who intend to enter upon some active business after graduation as can be induced to follow it.

Under such a plan we should have in Columbia College four different classes of students: (1) those who were taking the shorter course of two years in preparation for a technical and professional course, and who would therefore look forward to a total university residence of five or six years; (2) those who were taking the shorter course of two years, but without any thought of subsequent professional or technical study; (3) those who felt able to give the time necessary to take the longer course of four years before entering a professional or technical school; and (4) those who, as now, take the four years' college course without any intention of technical or professional study. The second class of students would be a new and highly desirable class, and would be, for the most part, made up of earnest young men seeking a wider and more thorough scholarly training than the secondary school can offer, but unable to devote four years to that end. The third class of students would be able, by a proper selection of studies in the later years of their college course, either to enter a professional school with advanced standing or to anticipate some of the preliminary professional studies and to devote the time

so gained to more intensive professional work. Undoubtedly many students who now take a four years' undergraduate course with no professional or technical end in view would take the shorter course, and that only, but, on the other hand, numbers of students would come to college for a course of two years who when obliged to choose between a four years' course and none at all are compelled to give up college altogether. The final result of the changes would certainly be to increase the total number of students taking a college course of one length or another.

The dean of Columbia College is of the opinion that such a shortened course of two years as is contemplated by this suggestion could readily be made to include all the studies now prescribed at Columbia for candidates for the degree of bachelor of arts. This shortened course would, therefore, take on something of the definitiveness and purpose which in many cases the rapid developments of recent years have removed from undergraduate study; for it goes without saying that no effort would be spared to make such a two years' course as valuable as possible, both for intellectual training and for the development of character. The student would be a gainer, not a loser, by the change.

THE DEGREES OF BACHELOR OF ARTS AND OF MASTER OF ARTS.

If Columbia College should offer two courses in the liberal arts and sciences, one of two years and one of four years in length, the second including the first, the question would at once arise as to what degrees or other marks of academic recognition would be conferred upon students who had satisfactorily completed them.

Two answers appear to be possible. First, we may withhold the bachelor's degree until the completion of the longer course, and grant some new designation to those who satisfactorily complete the shorter course. This has been done at the University of Chicago, where graduates of the junior college course of two years are made associates in arts. Or we may degrade—as it is called—the bachelor's degree from the artificial position in which the developments of the last forty years have placed it, and confer it upon the graduates of the shorter course of two years, and give the degree of master of arts for the longer course of four years. The latter alternative would be my own preference. Such a plan would bring the degree of bachelor of arts two years earlier than now and would place it substantially on a par with the bachelor's degree in France, the *Zeugniss der Reife* in Germany, and the ordinary degree in course as conferred by the English and the Scottish universities. It would also be substantially on a par with the Columbia College degree of 1860.

In this connection it must be remembered that it is not the A.B. degree of to-day which is so much extolled and so highly esteemed as the mark of a liberal education gained by hard study and severe discipline, but that of one and two generations ago. The A.B. degree of to-day is a very uncertain quantity, and time alone will show whether it means much or little.

The degree of master of arts is an entirely appropriate reward for the completion of a college course, under the new conditions proposed, four years in length. This degree has been put to many varied uses and has no generally accepted significance. In Scotland it is given in place of the degree of bachelor of arts at the close of three very short years of undergraduate study. In England it signifies that the holder is a bachelor of arts, that he has lived for a certain minimum number of terms after obtaining the bachelor's degree, and that he has paid certain fees.

In Germany it is usually included in the degree of doctor of philosophy. In the United States the degree is more often than not a purely honorary designation; although in recent years the stronger universities have guarded it strictly and now grant it for a minimum period of graduate study for one year in residence. At the meeting of the Association of American Universities in February last there was a very interesting discussion on the subject of this degree, and the divergence of policy in regard to it was made plainly evident. As an intermediate degree between those of bachelor of arts and doctor of philosophy, that of master of arts has been and is very useful at Columbia. It marks the close of a period of serious resident graduate study, and is an appropriate reward for the work of those university students who have neither the inclination nor the peculiar abilities and temperament to fit themselves for successful examination for the degree of doctor of philosophy. At the same time it must be admitted that the rapid development of the elective system and the widely different standards of the scores of colleges from which our graduate students come, have almost wiped out the distinction between the senior year in Columbia College and the first year of graduate study. To the best of my knowledge and belief, the fixing of the degree of master of arts at the close of a four years' undergraduate course would involve no real alteration in the standard required on the part of those coming to Columbia from other institutions. For students of Columbia College it would bring the degree within reach after four years of residence instead of five.

In the case of candidates for the degree of doctor of philosophy, the completion of the longer college course, or its equivalent elsewhere, would of course be required, and also the same minimum period of post-graduate resident study as now. There would be no alteration in the time necessary or the standard now set for that degree, which as conferred at Columbia is recognized as conforming to the highest and best standards.

With the courses in applied science and in medicine fixed at four years, to base them upon a two years' college course would be to elevate them to a proper university standard and to ensure the best possible class of students. The Law School and the professional course in Teachers College could easily be put upon the same basis.

Reflection and a careful study of the facts will make it apparent that these suggestions are less radical than seems to be the case on first sight. They at least offer a solution to a generally recognized problem, one which has often been pointed to but toward the solution of which little progress has been made. I shall seek an early opportunity of bringing them before the university council and the several faculties for full consideration and discussion.

THE FUTURE OF THE AMERICAN COLLEGE.

Should Columbia University adopt such a policy as has been outlined, and should the same or a similar policy commend itself to the governing bodies of any other American universities whose problems are similar to ours, a development already in progress throughout the country would be hastened. As the public high schools multiply and strengthen they will tend more and more to give the instruction now offered in the first year, or first two years, of the college course. In so far, they will become local colleges, but without the characteristic or the attractiveness of student residence. Furthermore, the time would sooner come when colleges, excellent in ideals and rich in teaching power but without the resources necessary to carry

on a four years' course of instruction satisfactorily, will raise the requirements for admission to a proper point and then concentrate all their strength upon a thoroughly sound course of two years leading to the bachelor's degree. More depends upon the strict enforcement of proper standards of admission to college than is generally believed; that is at present the weakest point in college administration. The general standard of college education in the United States would be strengthened more if the weaker colleges would fix and rigidly enforce proper entrance requirements and concentrate all their money and energies upon two years of thorough college work than if they continue to spread a college course over four years with admission secured on nominal terms or on none at all.

The policy outlined would, I think, largely increase the number of students seeking a college education, and many who might enter one of the stronger colleges for the two years' course would remain for four years. The loss of income due to the dropping out of students after two years of residence would be more than made good very soon by the large increase in college attendance.

As the system of higher education in the United States has developed it has become apparent that we have substituted three institutions—secondary school, college and university—for the two—secondary school and university—which exist in France and Germany. The work done in the United States by the best colleges is done in France and Germany one half by the secondary school and one half by the university. The training given in Europe differs in many ways from that given here, but from an administrative point of view the comparison just made is substantially correct. The college, as we have it, is peculiar to our own national system of education, and is perhaps its strongest, as it certainly is its most characteristic, feature. It breaks the sharp transition which is so noticeable in Europe between the close surveillance and prescribed order of the secondary school and the absolute freedom of the university. Its course of liberal study comes just at the time in the student's life to do him most good, to open and inform his intelligence and to refine and strengthen his character. Its student life, social opportunities, and athletic sports are all additional elements of usefulness and of strength. It has endeared itself to three or four generations of the flower of our American youth and it is more useful to-day than at any earlier time.

For all of these reasons I am anxious to have it preserved as part of our educational system and so adjusted to the social and educational conditions which surround us that a college training may be an essential part of the higher education of an American whether he is destined to a professional career or to a business occupation. It seems to me clear that if the college is not so adjusted it will, despite its recent rapid growth, lose its prestige and place of honor in our American life, and that it may eventually disappear entirely, to the great damage of our whole educational system.

NICHOLAS MURRAY BUTLER.

ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

III.

THE UPPER AIR AND AURORAS.

THE present liquid ocean, neglecting everything for the moment but the water, was at a previous period of the earth's history part of the atmosphere, and its condensation has been brought about by the gradual cooling of the earth's surface. This resulting ocean is subjected to the pressure of the remaining uncondensed

gases, and as these are slightly soluble they dissolve to some extent in the fluid. The gases in solution can be taken out by distillation or by exhausting the water, and if we compare their volume with the volume of the water as steam, we should find about one volume of air in 60,000 volumes of steam. This would then be about the rough proportion of the relatively permanent gas to condensable gas which existed in the case of the vaporized ocean. Now let us assume the surface of the earth gradually cooled to some 200 degrees below the freezing-point; then, after all the present ocean was frozen, and the climate became three times more intense than any arctic frost, a new ocean of liquid air would appear, covering the entire surface of the frozen globe about thirty-five feet deep. We may now apply the same reasoning to the liquid air ocean that we formerly did to the water one, and this would lead us to anticipate that it might contain in solution some gases that may be far less condensable than the chief constituents of the fluid. In order to separate them we must imitate the method of taking the gases out of water. Assume a sample of liquid air cooled to the low temperature that can be reached by its own evaporation, connected by a pipe to a condenser cooled in liquid hydrogen; then any volatile gases present in solution will distil over with the first portions of the air, and can be pumped off, being uncondensable at the temperature of the condenser. In this way, a gas mixture, containing, of the known gases, free hydrogen, helium and neon, has been separated from liquid air. It is interesting to note in passing that the relative volatilities of water and oxygen are in the same ratio as those of liquid air and hydrogen, so that the analogy between the ocean of water and that of liquid air has another suggestive parallel. The total uncondensable gas separated in this way amounts to about one fifty-thousandth of the volume of the air, which is about the same proportion as the air dissolved in water. That free hydrogen exists in air in small amount is conclusively proved, but the actual proportion found by the process is very much smaller than Gautier has estimated by the combustion method. The recent experiments of Lord Rayleigh show that Gautier, who estimated the hydrogen present as one five-thousandth, has in some way produced more hydrogen than he can manage to extract from pure air by a repetition of the same process. The spectroscopic examination of these gases throws new light upon the question of the aurora and the nature of the upper air. On passing electric discharges through the tubes containing the most volatile of the atmospheric gases, they glow with a bright orange light, which is especially marked at the negative pole. The spectroscope shows that this light consists, in the visible part of the spectrum, chiefly of a succession of strong rays in the red, orange and yellow, attributed to hydrogen, helium and neon. Besides these, a vast number of rays, generally less brilliant, are distributed through the whole length of the visible spectrum. The greater part of these rays are of, as yet, unknown origin. The violet and ultra-violet part of the spectrum rivals in strength that of the red and yellow rays. As these gases probably include some of the gases that pervade interplanetary space, search was made for the prominent nebular, coronal and auroral lines. No definite lines agreeing with the nebular spectrum could be found, but many lines occurred closely coincident with the coronal and auroral spectrum. But before discussing the spectroscopic problem it will be necessary to consider the nature and condition of the upper air.

According to the old law of Dalton, supported by the modern dynamical theory of

gases, each constituent of the atmosphere while acted upon by the force of gravity forms a separate atmosphere, completely independent, except as to temperature, of the others, and the relations between the common temperature and the pressure and altitude for each specific atmosphere can be definitely expressed. If we assume the altitude and temperature known, then the pressure can be ascertained for the same height in the case of each of the gaseous constituents, and in this way the percentage composition of the atmosphere at that place may be deduced. Suppose we start with a surface atmosphere having the composition of our air, only containing two ten-thousandths of hydrogen, then at thirty-seven miles, if a sample could be procured for analysis, we believe that it would be found to contain 12 per cent. of hydrogen and only 10 per cent. of oxygen. The carbonic acid practically disappears; and by the time we reach forty-seven miles, where the temperature is *minus* 132 degrees, assuming a gradient of 3.2 degrees per mile, the nitrogen and oxygen have so thinned out that the only constituent of the upper air which is left is hydrogen. If the gradient of temperature were doubled, the elimination of the nitrogen and oxygen would take place by the time thirty-seven miles was reached, with a temperature of *minus* 220 degrees. The permanence of the composition of the air at the highest altitudes, as deduced from the basis of the dynamical theory of gases, has been discussed by Stoney, Bryan and others. It would appear that there is a consensus of opinion that the rate at which gases like hydrogen and helium could escape from the earth's atmosphere would be excessively slow. Considering that to compensate any such loss the same gases are being supplied by actions taking place in the crust of the earth, we may safely regard them as necessarily permanent constituents of the upper air. The temperature at the elevations we have been discussing would not be sufficient to cause any liquefaction of the nitrogen and oxygen, the pressure being so low. If we assume the mean temperature as about the boiling-point of oxygen at atmospheric pressure, then a considerable amount of the carbonic acid must solidify as a mist, if the air from a lower level be cooled to this temperature; and the same result might take place with other gases of relatively small volatility which occur in air. This would explain the clouds that have been seen at an elevation of fifty miles, without assuming the possibility of water vapor being carried up so high. The temperature of the upper air must be above that on the vapor pressure curve corresponding to the barometric pressure at the locality, otherwise liquid condensation must take place. In other words, the temperature must be above the dew-point of air at that place. At higher elevations, on any reasonable assumption of temperature distribution, we inevitably reach a temperature where the air would condense, just as Fourier and Poisson supposed it would, unless the temperature is arrested in some way from approaching the zero. Both ultra-violet absorption and the prevalence of electric storms may have something to do with the maintenance of a higher mean temperature. The whole mass of the air above forty miles is not more than one seven-hundredth part of the total mass of the atmosphere, so that any rain or snow of liquid or solid air, if it did occur, would necessarily be of a very tenuous description. In any case, the dense gases tend to accumulate in the lower strata, and the lighter ones to predominate at the higher altitudes, always assuming that a steady state of equilibrium has been reached. It must be observed, however, that a sample of air taken at an elevation of nine miles

has shown no difference in composition from that at the ground, whereas, according to our hypothesis, the oxygen ought to have been diminished to 17 per cent., and the carbonic acid should also have become much less. This can only be explained by assuming that a large intermixture of different layers of the atmosphere is still taking place at this elevation. This is confirmed by a study of the motions of clouds about six miles high, which reveals an average velocity of the air currents of some seventy miles an hour; such violent winds must be the means of causing the intermingling of different atmospheric strata. Some clouds, however, during hot and thundery weather, have been seen to reach an elevation of seventeen miles, so that we have direct proof that on occasion the lower layers of atmosphere are carried to a great elevation. The existence of an atmosphere at more than a hundred miles above the surface of the earth is revealed to us by the appearance of meteors and fireballs, and when we can take photographs of the spectrum of such apparitions we shall learn a great deal about the composition of the upper air. In the meantime Pickering's solitary spectrum of a meteor reveals an atmosphere of hydrogen and helium, and so far this is corroborative of the doctrine we have been discussing. It has long been recognized that the aurora is the result of electric discharges within the limits of the earth's atmosphere, but it was difficult to understand why its spectrum should be so entirely different from anything which could be produced artificially by electric discharges through rarefied air at the surface of the earth. Writing in 1879, Rand Capron, after collecting all the recorded observations, was able to enumerate no more than nine auroral rays, of which but one could with any probability be identified with rays emitted by atmospheric air under an electric discharge. Vogel attributed this want of agreement between nature and experiment, in a vague way, to difference of temperature and pressure; and Zollner thought the auroral spectrum to be one of a different order, in the sense in which the line and band spectra of nitrogen are said to be of different orders. Such statements were merely confessions of ignorance. But since that time observations of the spectra of auroras have been greatly multiplied, chiefly through the Swedish and Danish Polar Expeditions, and the length of spectrum recorded on the ultra-violet side has been greatly extended by the use of photography, so that, in a recent discussion of the results, M. Henri Stassano is able to enumerate upwards of one hundred auroral rays, of which the wave-length is more or less approximately known, some of them far in the ultra-violet. Of this large number of rays he is able to identify, within the probable limits of errors of observation, about two thirds as rays, which Professor Liveing and myself have observed to be emitted by the most volatile gases of atmospheric air unliquefiable at the temperature of liquid hydrogen. Most of the remainder he ascribes to argon, and some he might, with more probability, have identified with krypton or xenon rays, if he had been aware of the publication of wave-lengths of the spectra of those gases, and the identification of one of the highest rays of krypton with that most characteristic of auroras. The rosy tint often seen in auroras, particularly in the streamers, appears to be due mainly to neon, of which the spectrum is remarkably rich in red and orange rays. One or two neon rays are amongst those most frequently observed, while the red ray of hydrogen and one red ray of krypton have been noticed only once. The predominance of neon is not surprising, seeing that from its relatively greater proportion in air and its low den-

sity it must tend to concentrate at higher elevations. So large a number of probable identifications warrants the belief that we may yet be able to reproduce in our laboratories the auroral spectrum in its entirety. It is true that we have still to account for the appearance of some, and the absence of other, rays of the newly discovered gases, which in the way in which we stimulate them appear to be equally brilliant, and for the absence, with one doubtful exception, of all the rays of nitrogen. If we cannot give the reason of this, it is because we do not know the mechanism of luminescence—nor even whether the particles which carry the electricity are themselves luminous, or whether they only produce stresses causing other particles which encounter them to vibrate; yet we are certain that an electric discharge in a highly rarefied mixture of gases lights one element and not another, in a way which, to our ignorance, seems capricious. The Swedish North Polar Expedition concluded from a great number of trigonometrical measurements that the average above the ground of the base of the aurora was fifty kilometers (thirty-four miles) at Cape Thorsden, Spitzbergen; at this height the pressure of the nitrogen of the atmosphere would be only about one tenth of a millimeter, and Moissan and Deslandres have found that in atmospheric air at pressures less than one millimeter the rays of nitrogen and oxygen fade and are replaced by those of argon and by five new rays which Stassano identifies with rays of the more volatile gases measured by us. Also Collie and Ramsay's observations on the distance to which electrical discharges of equal potential traverse different gases explosively throw much light on the question; for they find that, while for helium and neon this distance is from 250 to 300 mm., for argon it is 45½ mm., for hydrogen it is 39 mm., and for air and oxygen still less. This indicates that a good deal depends on the very constitution of the gases themselves, and certainly helps us to understand why neon and argon, which exist in the atmosphere in larger proportions than helium, krypton or xenon, should make their appearance in the spectrum of auroras almost to the exclusion of nitrogen and oxygen. How much depends not only on the constitution and it may be temperature of the gases, but also on the character of the electric discharge, is evident from the difference between the spectra at the cathode and anode in different gases, notably in nitrogen and argon, and not less remarkably in the more volatile compounds of the atmosphere. Paulsen thinks the auroral spectrum wholly due to cathodic rays. Without stopping to discuss that question, it is certain that changes in the character of the electric discharge produce definite changes in the spectra excited by them. It has long been known that in many spectra the rays which are inconspicuous with an uncondensed electric discharge become very pronounced when a Leyden jar is in the circuit. This used to be ascribed to a higher temperature in this condensed spark, though measurements of that temperature have not borne out the explanation. Schuster and Hemsalech have shown that these changes of spectra are in part due to the oscillatory character of the condenser discharge which may be enhanced by self-induction, and the corresponding change of spectrum thereby made more pronounced. Lightning we should expect to resemble condensed discharge much more than aurora, but this is not borne out by the spectrum. Pickering's recent analysis of the spectrum of a flash obtained by photography shows, out of nineteen lines measured by him, only two which can be assigned with probability to nitrogen and oxygen, while three hydrogen rays most likely due to water are very conspicu-

ous, and eleven may be reasonably ascribed to argon, krypton and xenon, one to more volatile gas of the neon class, and the brightest ray of all is but a very little less refrangible than the characteristic auroral ray, and coincides with a strong ray of calcium, but also lies between, and close to, an argon and a neon ray, neither of them weak rays. There may be some doubt about the identification of the spectral rays of auroras because of the wide limits of the probable errors in measuring wave-lengths so faint as most of them are, but there is no such doubt about the wave-lengths of the rays in solar protuberances measured by Deslandres and Hale. Stassano found that these rays, 44 in number, lying between the Fraunhofer line *F* and 3,148 in the ultra-violet agree very closely with rays which Professor Liveing and myself measured in the spectra of the most volatile atmospheric gases. It will be remembered that one of the earliest suggestions as to the nature of solar prominences was that they were solar auroras. This supposition helped to explain the marvellous rapidity of their changes, and the apparent suspension of brilliant self-luminous clouds at enormous heights above the sun's surface. Now the identification of the rays of their spectra with those of the most volatile gases, which also furnish many of the auroral rays, certainly supports that suggestion. A stronger support, however, seems to be given to it by the results obtained at the total eclipse of May, 1901, by the American expedition to Sumatra. In the *Astrophysical Journal* for June last is a list of 339 lines in the spectrum of the corona photographed by Humphreys, during totality, with a very large concave grating. Of these no fewer than 209 do not differ from lines we have measured in the most volatile gases of the atmosphere, or in krypton or xenon, by more than one unit of wave-length on Armstrong's scale, a quantity within the limit of probable error. Of the remainder, a good many agree to a like degree with argon lines, a very few with oxygen lines, and still fewer with nitrogen lines; the characteristic green auroral ray, which is not in the range of Humphreys' photographs, also agrees within a small fraction of a unit of wave-length with one of the rays emitted by the most volatile atmospheric gas. Taking into account the Fraunhofer lines *H*, *K* and *G*, usually ascribed to calcium, there remain only fifty-five lines of the 339 unaccounted for to the degree of probability indicated. Of these considerably more than half are very weak lines which have not depicted themselves on more than one of the six films exposed, and extend but a very short distance into the sun's atmosphere. There are, however, seven which are stronger lines, and reach to a considerable height above the sun's rim, and all have depicted themselves on at least four of the six films. If there be no considerable error in the wave-lengths assigned (and such is not likely to be the case), these lines may perhaps be due to some volatile element which may yet be discovered in our atmosphere. However that may be, the very great number of close coincidences between the auroral rays and those which are emitted under electric excitement by gases of our atmosphere almost constrains us to believe, what is indeed most probable on other grounds, that the sun's coronal atmosphere is composed of the same substances as the earth's and that it is rendered luminous in the same way—namely, by electric discharges. This conclusion has plainly an important bearing on the explanation which should be given of the outburst of new stars and of the extraordinary and rapid changes in their spectra. Moreover, leaving on one side the question whether gases ever become luminous by the direct action of

heat, apart from such transfers of energy as occur in chemical change and electric disturbance, it demands a revision of the theories which attribute more permanent differences between the spectra of different stars to differences of temperature, and a fuller consideration of the question whether they cannot with better reason be explained by differences in the electric conditions which prevail in the stellar atmosphere.

If we turn to the question what is the cause of the electric discharges which are generally believed to occasion auroras, but of which little more has hitherto been known than that they are connected with sunspots and solar eruptions, recent studies of electric discharges in high vacua, with which the names of Crookes, Röntgen, Lenard and J. J. Thomson will always be associated, have opened the way for Arrhenius to suggest a definite and rational answer. He points out that the frequent disturbances which we know to occur in the sun must cause electric discharges in the sun's atmosphere far exceeding any that occur in that of the earth. These will be attended with an ionization of the gases, and the negative ions will stream away through the outer atmosphere of the sun into the interplanetary space, becoming, as Wilson has shown, nuclei of aggregation of condensable vapors and cosmic dust. The liquid and solid particles thus formed will be of various sizes; the larger will gravitate back to the sun, while those with diameters less than one and a half thousandths of a millimeter, but nevertheless greater than a wave-length of light, will, in accordance with Clerk-Maxwell's electromagnetic theory, be driven away from the sun by the incidence of the solar rays upon them, with velocities which may become enormous, until they meet other celestial bodies, or increase their dimensions by picking up more cosmic dust or diminish them by evaporation. The earth will catch its share of such particles on the side which is turned towards the sun, and its upper atmosphere will thereby become negatively electrified until the potential of the charge reaches such a point that a discharge occurs, which will be repeated as more charged particles reach the earth. This theory not only accounts for the auroral discharges, and the coincidence of their times of greatest frequency with those of the maxima of sunspots, but also for the minor maxima and minima. The vernal and autumnal maxima occur when the line through the earth and sun has its greatest inclination to the solar equator, so that the earth is more directly exposed to the region of maximum of sunspots, while the twenty-six days' period corresponds closely with the period of rotation of that part of the solar surface where faculæ are most abundant. J. J. Thomson has pointed out, as a consequence of the Richardson observations, that negative ions will be constantly streaming from the sun merely regarded as a hot body, but this is not inconsistent with the supposition that there will be an excess of this emission in eruptions, and from the regions of faculæ. Arrhenius' theory accounts also, in a way which seems the most satisfactory hitherto enunciated, for the appearances presented by comets. The solid parts of these objects absorb the sun's rays, and as they approach the sun become heated on the side turned towards him until the volatile substances frozen in or upon them are evaporated and diffused in the gaseous state in surrounding space, where they get cooled to the temperature of liquefaction and aggregated in drops about the negative ions. The larger of these drops gravitate towards the sun and form clouds of the coma about the head, while the smaller are driven by the incidence of the sun's light upon them away from the sun and form the tail. The

curvature of the tail depends, as Bredichin has shown, on the rate at which the particles are driven, which in turn depends on the size and specific gravity of the particles, and these will vary with the density of the vapor from which they are formed and the frequency of the negative ions which collect them. In any case Arrhenius' theory is a most suggestive one, not only with reference to auroras and comets, and the solar corona and chromosphere, but also as to the constitution of the photosphere itself.

VARIOUS LOW-TEMPERATURE RESEARCHES.

We may now summarize some of the results which have already been attained by low-temperature studies. In the first place, the great majority of chemical interactions are entirely suspended, but an element of such exceptional powers of combination as flourine is still active at the temperature of liquid air. Whether solid fluorine and liquid hydrogen would interact no one can at present say. Bodies naturally become denser, but even a highly expansive substance like ice does not appear to reach the density of water at the lowest temperature. This is confirmatory of the view that the particles of matter under such conditions are not packed in the closest possible way. The force of cohesion is greatly increased at low temperatures, as is shown by the additional stress required to rupture metallic wires. This fact is of interest in connection with two conflicting theories of matter. Lord Kelvin's view is that the forces that hold together the particles of bodies may be accounted for without assuming any other agency than gravitation or any other law than the Newtonian. An opposite view is that the phenomena of the aggregation of molecules depend upon the molecular vibration as a physical cause. Hence, at the zero of absolute temperature, this vibrating energy being in complete abeyance, the phenomena of cohesion should cease to exist, and matter generally be reduced to an incoherent heap of cosmic dust. This second view receives no support from experiment.

The photographic action of light is diminished at the temperature of liquid air to about twenty per cent. of its ordinary efficiency, and at the still lower temperature of liquid hydrogen only about ten per cent. of the original sensitivity remains. At the temperature of liquid air or liquid hydrogen a large range of organic bodies and many inorganic ones acquire under exposure to violet light the property of phosphorescence. Such bodies glow faintly so long as they are kept cold, but become exceedingly brilliant during the period when the temperature is rising. Even solid air is a phosphorescent body. All the alkaline earth sulphides which phosphoresce brilliantly at the ordinary temperature lose this property when cooled, to be revived on heating; but such bodies in the first instance may be stimulated through the absorption of light at the lowest temperatures. Radio-active bodies, on the other hand, like radium, which are naturally self-luminous, maintain this luminosity unimpaired at the very lowest temperatures, and are still capable of inducing phosphorescence in bodies like the platino-cyanides. Some crystals become for a time self-luminous when cooled in liquid air or hydrogen, owing to the induced electric stimulation causing discharges between the crystal molecules. This phenomenon is very pronounced with nitrate of uranium and some platino-cyanides.

In conjunction with Professor Fleming a long series of experiments was made on the electric and magnetic properties of bodies at low temperatures. The subjects that have been under investigation may be

classified as follows: 'The Thermo-Electric Powers of Pure Metals'; 'The Magnetic Properties of Iron and Steel'; 'Dielectric Constants'; 'The Magnetic and Electric Constants of Liquid Oxygen'; 'Magnetic Susceptibility.'

The investigations have shown that electric conductivity in pure metals varies almost inversely as the absolute temperature down to *minus* 200 degrees, but that this law is greatly affected by the presence of the most minute amount of impurity. Hence the results amount to a proof that electric resistance in pure metals is closely dependent upon the molecular or atomic motion which gives rise to temperature, and that the process by which the energy constituting what is called an electric current is dissipated essentially depends upon non-homogeneity of structure and upon the absolute temperature of the material. It might be inferred that at the zero of absolute temperature resistance would vanish altogether, and all pure metals become perfect conductors of electricity. This conclusion, however, has been rendered very doubtful by subsequent observations made at still lower temperatures, which appear to point to an ultimate finite resistance. Thus the temperature at which copper was assumed to have no resistance was *minus* 223 degrees, but that metal has been cooled to *minus* 253 degrees without getting rid of all resistance. The reduction in resistance of some of the metals at the boiling-point of hydrogen is very remarkable. Thus copper has only one per cent., gold and platinum three per cent., and silver four per cent. of the resistance they possessed at zero C., but iron still retains twelve per cent. of its initial resistance. In the case of alloys and impure metals, cold brings about a much smaller decrease in resistivity, and in the case of carbon and insulators like guttapercha, glass, ebonite, etc., their resistivity steadily increases. The enormous increase in resistance of bismuth when transversely magnetized and cooled was also discovered in the course of these experiments. The study of dielectric constants at low temperatures has resulted in the discovery of some interesting facts. A fundamental deduction from Maxwell's theory is that the square of the refractive index of a body should be the same number as its dielectric constant. So far, however, from this being the case generally, the exceptions are far more numerous than the coincidences. It has been shown in the case of many substances, such as ice and glass, that an increase in the frequency of the alternating electromotive force results in a reduction of the dielectric constant to a value more consistent with Maxwell's law. By experiments upon many substances it is shown that even a moderate increase of frequency brings the large dielectric constant to values quite near to that required by Maxwell's law. It was thus shown that low temperature has the same effect as high frequency in annulling the abnormal dielectric values. The exact measurement of the dielectric constant of liquid oxygen as well as its magnetic permeability, combined with the optical determination of the refractive index, showed that liquid oxygen strictly obeys Maxwell's electro-optic law even at very low electric frequencies. In magnetic work the result of greatest value is the proof that magnetic susceptibility varies inversely as the absolute temperature. This shows that the magnetization of paramagnetic bodies is an affair of orientation of molecules, and it suggests that at the absolute zero all the feebly paramagnetic bodies will be strongly magnetic. The diamagnetism of bismuth was found to be increased at low temperatures. The magnetic moment of a steel magnet is tempo-

rarily increased by cooling in liquid air, but the increase seems to have reached a limit, because on further cooling to the temperature of liquid hydrogen hardly any further change was observed. The study of the thermo-electric relations of the metals at low temperatures resulted in a great extension of the well-known Tait thermo-electric diagram. Tait found that the thermo-electric power of the metals could be expressed by a linear function of the absolute temperature, but at the extreme range of temperature now under consideration this law was found not to hold generally; and further, it appeared that many abrupt electric changes take place, which originate probably from specific molecular changes occurring in the metal. The thermo-electric neutral points of certain metals, such as lead and gold, which are located about or below the boiling-point of hydrogen, have been found to be a convenient means of defining specific temperatures in this exceptional part of the scale.

The effect of cold upon the life of living organisms is a matter of great intrinsic interest, as well as of wide theoretical importance. Experiment indicates that moderately high temperatures are much more fatal, at least to the lower forms of life, than are exceedingly low ones. Professor McKendrick froze for an hour at a temperature of 182° C. samples of meat, milk, etc., in sealed tubes; when these were opened after being kept at blood heat for a few days, their contents were found to be quite putrid. More recently some more elaborate tests were carried out at the Jenner Institute of Preventive Medicine on a series of typical bacteria. These were exposed to the temperature of liquid air for twenty hours, but their vitality was not affected, their functional activities remained unimpaired, and the cultures which they yielded were normal in every respect. The same result was obtained when liquid hydrogen was substituted for air. A similar persistence of life in seeds has been demonstrated even at the lowest temperatures; they were frozen for over a hundred hours in liquid air, at the instance of Messrs. Brown and Escombe, with no other result than to affect their protoplasm with a certain inertness, from which it recovered with warmth. Subsequently commercial samples of barley, pea, vegetable marrow, and mustard seeds were literally steeped for six hours in liquid hydrogen at the Royal Institution, yet when they were sown by Sir W. T. Thiselton Dyer at Kew in the ordinary way, the proportion in which germination occurred was no less than in the other batches of the same seeds which had suffered no abnormal treatment. Bacteria are minute vegetable cells, the standard of measurement for which is the 'mikron.' Yet it has been found possible to completely triturate these microscopic cells, when the operation is carried out at the temperature of liquid air, the cells then being frozen into hard breakable masses. The typhoid organism has been treated in this way, and the cell plasma obtained for the purpose of studying its toxic and immunizing properties. It would hardly have been anticipated that liquid air should find such immediate application in biological research. A research by Professor Macfadyen, just concluded, has shown that many varieties of microorganisms can be exposed to the temperature of liquid air for a period of six months without any appreciable loss of vitality, although at such a temperature the ordinary chemical processes of the cell must cease. At such a temperature the cells cannot be said to be either alive or dead, in the ordinary acceptation of these words. It is a new and hitherto unobtained condition of living matter—a third state. A final in-

stance of the application of the above methods may be given. Certain species of bacteria during the course of their vital processes are capable of emitting light. If, however, the cells be broken up at the temperature of liquid air, and the crushed contents brought to the ordinary temperature, the luminosity function is found to have disappeared. This points to the luminosity not being due to the action of a ferment—a 'Luciferase'—but as being essentially bound up with the vital processes of the cells, and dependent for its production on the intact organization of the cell. These attempts to study by frigorific methods the physiology of the cell have already yielded valuable and encouraging results, and it is to be hoped that this line of investigation will continue to be vigorously prosecuted at the Jenner Institute.

And now, to conclude an address which must have sorely taxed your patience, I may remind you that I commenced by referring to the plaint of Elizabethan science, that cold was not a natural available product. In the course of a long struggle with nature, man, by the application of intelligent and steady industry, has acquired a control over this agency which enables him to produce it at will, and with almost any degree of intensity, short of a limit defined by the very nature of things. But the success in working what appears, at first sight, to be a quarry of research that would soon suffer exhaustion, has only brought him to the threshold of new labyrinths, the entanglements of which frustrate, with a seemingly invulnerable complexity, the hopes of further progress. In a legitimate sense all genuine scientific workers feel that they are 'the inheritors of unfulfilled renown.' The battlefields of science are the centers of a perpetual warfare, in which there is no hope of final victory, although partial conquest is ever triumphantly encouraging the continuance of the disciplined and strenuous attack on the seemingly impregnable fortress of Nature. To serve in the scientific army, to have shown some initiative, and to be rewarded by the consciousness that in the eyes of his comrades he bears the accredited accolade of successful endeavor, is enough to satisfy the legitimate ambition of every earnest student of Nature. The real warranty that the march of progress in the future will be as glorious as in the past lies in the perpetual reinforcement of the scientific ranks by recruits animated by such a spirit, and proud to obtain such a reward.

JAMES DEWAR.

SCIENTIFIC BOOKS.

Notes on Naval Progress. July, 1902. Office of Naval Intelligence. Washington, Government Printing Office. 1902. 8vo. Paper. Pp. 502; over 100 illustrations, plates, maps, tables, etc.

This very large and exceedingly valuable document constitutes No. XXI., General Information Series, of the Office of Naval Intelligence, a division of the Naval Organization which has now for many years been justifying its existence by great and increasing efficiency. Under the supervision of Captain Sigsbee, the present Chief Intelligence Officer, it is evidently fully maintaining its standing. The contributors to this bulky volume are usually young officers of the navy who exhibit that talent for exact, concise and comprehensive composition which is the distinguishing characteristic of a good official report, and that excellence in style which seems so common a talent with military and naval officers. The two probably necessarily go together and are the outcome of familiarity with, often a minute study of, the reports and writings of great commanders quite as much as of careful drill at the governmental technical schools.

The volume in hand contains notes on ships and torpedo-boats, on ordnance and armor, on engineering progress, electricity, wireless

telegraphy, the naval manœuvres of 1901, the naval budgets of great powers for 1902–3, and on modern battle-ships, including particularly the *Vittorio Emanuele*. The papers are all written by experts in their several departments and are as full of information as is an egg of meat.

Foreign naval powers are still increasing the magnitude and the offensive and defensive values of their battle-ships and cruisers and the big British and French navies especially are making progress with their 'submarines' and their 'submersibles.' Both report favorably on the types already constructed and indicate steady improvement. The former is testing the Holland craft. 'No. 1' is afloat and performs well. She can travel four hundred miles unexposed to fire. A 'periscope' permits a lookout being kept when completely submerged. The French *Triton* made a twenty-four hour trial, largely submerged, and during a part of the time in bad weather, and worked well. Many torpedo-boat destroyers are reported as attaining thirty knots on their contract trials. These vessels seem to be subject to large risk of accident.

In ordnance the tendency continues toward larger sizes of quick-firing guns and toward greater length for all classes of ordnance. In armor, the progress reported is in the direction of more efficient hardening and of a reduction in the thickness demanded to resist a stated impact of shot. In small arms, the small calibers persist and the 'automatic' system of continuous self-operation is being steadily perfected. A smokeless powder is now adapted for each class of ordnance, large and small, and this kind of explosive has become standard. The chemist is still seeking new and still more manageable and powerful compositions. Capped projectiles for heavy ordnance are successful, and a new device permits the production of a dense smoke at the point of explosion of the shell to confuse the enemy and disconcert his batteries. Torpedoes are still holding an important place in the field of investigation as well as in warfare, and there are no indications of the abandonment of this weapon.

Water-tube boilers, high steam-pressures (fifteen to twenty atmospheres and upward), with triple and quadruple expansion engines, are the rule and triple screws are gaining ground under the stimulus of the example set by our own navy and the arguments of its Engineer-in-Chief, Admiral Melville. The steam-turbine is being steadily developed and reduced to useful service on a large scale in both the naval and the merchant service. Liquid fuels are being exploited, and coal-handling devices, for use at sea as well as in port, are being brought into practicable forms.

There has been 'a striking extension' of the use of electricity in the internal minor services of the naval vessels of all nations, for the distribution of light and in the operation of guns and of machinery generally. The alternating current does not seem as yet successful. Voltages are usually low, but with a tendency toward elevation above the usual standard, which is about 80 volts minimum. Voltages of 120 and upward have been employed with a tendency toward 200 as a maximum limit.

Wireless telegraphy has progressed wonderfully, particularly in its range of action. The system is still imperfect, but is constantly being brought into practicable and useful form. All nations are experimenting with one or another of five best-known systems.

Comparison of the type-ships of existing navies seems to be favorable to the naval engineering and architecture of the United States, as illustrated in its latest constructions; but it is evident that competition is developing sharply in all leading navies, and the outcome among those of the greater powers seems likely to prove to be almost as largely dependent upon the liberality permitted by the legislative department as upon the genius of engineers, constructors and combatant officers.

R. H. Thurston.

SCIENTIFIC JOURNALS AND ARTICLES.

The closing (October) number of volume 3 of the *Transactions* of the American Mathematical Society contains the following papers: 'On the groups of order p^m, which contain operators of order p^{m-2},' by G. A. Mil-

ler; 'On the circuits of plane curves,' by C. A. Scott; 'Note on the real inflexions of plane curves,' by C. A. Scott; 'La théorie des plaques élastiques planes,' by J. Hadamard; 'Covariants of systems of linear differential equations and applications to the theory of ruled surfaces,' by E. J. Wilczynski; 'On the rank, order and class of algebraic minimum curves,' by A. S. Gale; 'On superosculating quadric surfaces,' by H. Maschke; 'Algebraic transformations of a complex variable realized by linkages,' by A. Emch; 'On the determination of the distance between two points in space of *m* dimensions,' by H. F. Blichfeldt; 'A definition of abstract groups,' by E. H. Moore; notes and errata: volumes 1, 2, 3.

THE opening (October) number of volume 9 of the *Bulletin* of the American Mathematical Society contains: 'Some instructive examples in the calculus of variations,' by Oskar Bolza; 'On the sufficient conditions in the calculus of variations,' by E. R. Hedrick; 'Some recent books on mechanics,' by E. B. Wilson; 'On a new edition of Stolz's Allgemeine Arithmetik, with an account of Peano's definition of number,' by E. V. Huntington; 'Lazarus Fuchs,' by E. J. Wilczynski; 'Notes'; 'New Publications.' The November *Bulletin* contains: 'The Ninth Summer Meeting of the American Mathematical Society,' by Edward Kasner; 'The Meeting of Section A of the American Association for the Advancement of Science, Pittsburgh, Pa., June 28 to July 3, 1902,' by E. S. Crawley; 'Second report on recent progress in the theory of groups of finite order,' by G. A. Miller; 'Shorter Notices'; 'Notes'; 'New Publications.'

THE September number of the *Botanical Gazette* contains the following papers: Dr. E. B. Copeland begins an historical and critical discussion of 'The Rise of the Transpiration Stream,' based upon an extended series of experiments that he carried on at the Hull Botanical Laboratory. The paper will be noticed more fully upon its completion. Harley P. Chamder publishes a revision of *Nemophila*, a genus which has occasioned considerable difference of opinion among Californian botanists. The author defines eighteen species and varieties, giving full discussion of critical points, synonymy, and citation of collections. Mr. W. C. Worsdell gives an account of his views concerning 'The Evolution of the Vascular Tissue of Plants,' beginning with the solid stele, which he thinks was derived from some bryophytic ancestry, and which is displayed among the most primitive ferns, and also in the juvenile stages of all ferns. The various stages in the evolution of the vascular tissue from this condition the author describes and illustrates. Professor Conway MacMillan suggests a classification of seeds in accordance with modern ideas of their structure and function. He gives general, structural, and genetic classifications. D. G. Fairchild describes *Mimosa pudica* as a weed in Ceylon, and reproduces a photograph of a large patch of it between Peradeniya and Colombo.

SHORTER ARTICLES.

ON THE STRUCTURE OF THE NUCLEUS.

1. HITHERTO the only irrefragable evidence showing that condensation is promoted by ionization, or in other words that negative ions are somewhat more active as condensation nuclei than positive ions, is the brilliant experiment devised by C. T. R. Wilson.* Nuclei are here produced by the X-rays in communicating condensation chambers, on the two sides of a vertical earthed metal plate, which receives electrical current normally on one side, through the ionized air, saturated with water vapor, and transmits the current in the same way and through the same medium on the other side. Necessarily there was an excess of negative ions on one side of the plate and an excess of positive ions on the other side. It was found, on producing condensation by exhaustion simultaneously on both sides under like conditions, that the fogs subsided on the positive side many times as rapidly as they did on the negative side, or that the negative ions are in correspondingly greater number. The effect is increasingly marked for smaller supersaturations.

2. On extending my work with shaken nuclei to solutions of non-conductors in non-conductors, such as naphthalene and of paraf-

* *Phil. Trans. Lond.*, Vol. 193, pp. 289–308, 1899.

fine in benzol, etc., I obtained results leading to the same interpretation as those already summarized for aqueous saline solutions in my last article. The nucleus is to be regarded as an exceedingly small droplet of concentrated solution, which persists, inasmuch as the decreased vapor pressure due to solution, at a certain specific radius, is exactly counterbalanced by the increased vapor pressure due to convexity. Thus, as my direct experiments have long ago shown, the nucleus depends for its size, cæt. par. on the medium in which it is produced or is generated; or in other words, on the medium into which any emanation is introduced or is generated. For if the nuclei are solutions, then the critical density and the diameter at which evaporation ceases for a given nucleus will depend on the quantity and kind of solute entrapped and on the vapor pressure equation in the broadest sense (involving temperature, surface tension, densities, etc.) of the given medium.

If this is true, then it seems doubtful to my mind whether the experiments of C. T. R. Wilson on the specific condensation effect of ionization can further be regarded as crucial.

3. If one introduces nuclei or makes nuclei by aid of the X-rays, in what is virtually the acid and the alkaline side of a battery, even if the ionized moist air is the electrolyte, one is surely conveying nuclei into, or making nuclei out of, different media. The stuff out of which solutes are to be fashioned may be available in different degrees on the two sides. Whatever chemical effect is produced on one side by the rays, need not at all be the same as on the other side, any more than the effect of shaking a very dilute solution need be the same as the effect of shaking a stronger solution, where the results have been shown to be enormously different as to the number, the velocity and persistence of nuclei produced. Hence from the accumulating evidence which I have brought forward, I am led to infer that the two species of nuclei in Wilson's experiment are, for mere chemical reasons, liable to be of different degrees of permanence, sizes and numbers, quite apart from the electric circumstances involved. One cannot, therefore, affirm that the difference (respectively positive and negative) of ionization is the immediate and sole cause of the difference of precipitation rates specified, or briefly that negative ions precipitate more effectively than positive ions, because both a difference of ionization and a chemical difference is involved; and the right to assert that ionization and not the chemical difference is the *vera causa* may be called in question, when in every other case the phenomena may be explained in terms of the latter.

I refer, of course, to immediate causes. Remotely, affinities and cohesions have the well-known electrical relations; but with remote causes I am not here concerned.

4. Finally, if a marked difference in efficiency as condensation nuclei of positive and negative ions is granted, then any ionized emanation, *neutral as a whole,* like that from phosphorus, should produce two groups of nuclei. On condensation there should be two groups of coronal particles, interpenetrating and subsiding through each other in the way I have frequently witnessed in other experiments. No such effect has been observed. Phosphorus nuclei are rather remarkable for their identity, and the regular coronas observed even after twenty-five or fifty exhaustions. If there is any variation of size of nucleus, it is graded as seen in the haziness of planes of demarcation after long lapses of time.

5. While these conclusions as to the origin of the different nuclei involve a theoretical difference from Wilson's deductions, they are not at variance with his practical conclusions; for if, through any radiation agency two different emanations are generated (with opposite charges or not), they would in a saturated medium correspond to two different nuclei, and the number of each kind and their diffusion rates in general, would also be different. If they should, at the same time, be opposed in ionization, a separation of charges will result. Indeed if two or more groups of ionized nuclei be generated in any manner whatever, they are liable to have different number and speed constants and lead to a separation of charges, be it only by diffusion.

But the case is much more definite, as the following paragraphs may indicate.

6. In this place I may again call attention to the fact that if retarded evaporation were effective in giving the nucleus permanence, if the observed dissipation of the nuclei of solutions were in any way dependent on evaporation, and not on the motion of nuclei, then those nuclei which are produced by shaking solutions of hygroscopic solutes like $CaCl_2$, H_2SO_4, etc., which can not wholly evaporate their water, should be more stable than other saline nuclei. The results show emphatically that this is not the case. Nuclei generated from hygroscopic bodies and their rate of evanescence is not exceptional and is no greater in the first case nor less in the second than that of saline bodies in general. Hence if the nucleus is necessarily a solution in case of the former solutes ($CaCl_2$, etc.), it is reasonable to suppose it always to be. I have found that the pressure decrement $\delta p < 2$ cm. of mercury (the limit is lower, but my apparatus in its present form does not allow me to go below this) is more than sufficient to precipitate the nuclei produced by shaking. For such small decrements the equation $.29\ \delta p/p = \delta\theta/(273+\theta)$, where θ is the temperature in degrees Centigrade, may be assumed. Now the decrement of vapor pressure, $\delta\pi$, corresponding to $\delta\theta$, is at 20° about $\delta\pi = .11\ \delta\theta$; whence $\delta\pi = .25$ cm. for the observed *excessive* δp. In other words, the vapor depression of a few millimeters is certainly much more than is required to stop the evaporation of the nucleus; so that if this depression is to be due to the solute, the solution need not even be very concentrated. For the case of H_2SO_4 at 20°, the nucleus would hold more than seventy-five per cent. of water in a saturated atmosphere, and at lower temperatures much greater dilution would suffice.

7. Inasmuch, therefore, as the nucleus, from my point of view, occurs under conditions of potential growth from a few molecules of dry solute to a relatively weak solution, as the air becomes more and more saturated, this growth and diminution must be a common occurrence in nature. The persistent atmospheric nuclei will be more dilute from the surface of the earth upward. The question then arises whether such growth or change of concentration is accompanied by electric charge quite apart from what is usually known as ionization (demonstrable presence of non-saturated chemical valencies); in other words, whether any change of size of these excessively fine particles is *reciprocally* accompanied by surface electrification. To be more specific: In an investigation published in 1892, Lenard showed that in presence of air pure water is electropositive, a circumstance which he attributes to a mere Volta contact effect. It needs but a trace of saline solute to reverse the potential. Solutions in presence of air are electronegative, and more so as a rule, as the concentration increases up to a definite value (6.5 per cent. in case of NaCl) for which the negative charge is a maximum. After this as concentration increases the potential gradually tends toward zero (attained for a solution stronger than about twenty-five per cent., in case of NaCl). Removal of nuclei by condensation and subsidence is then virtually a removal of negative electricity, provided the positive air charge is not simultaneously removed. Here then the possibility of a mechanism, in virtue of which growth or increasing dilution is associated with increased negative charge for the nucleus, is actually at hand; but the difficulty at present rests with the removal of the air charge.

8. Briefly then, the point which I wish to make is that the occurrence of charge is incident and not causal to the existence of the nucleus. What conditions its persistence and condensational activity is purely thermodynamic. What conditions the efficiency of electric transfer is a secondary property, open to investigation though as yet but little understood, and which even in the *same nucleus* is present in very variable amount or may even be quite absent. The phenomenon in its electric aspects depends, therefore, fundamentally on the critical density at which evaporation ceases.

In the above paragraphs I have endeavored to indicate how the current lines of argument bearing more or less remotely on atmospheric electricity at present stand; to point out that

none of them have as yet, to my thinking, been traced to an issue; and to show the direction in which I hope myself to contribute.

C. Barus.

Brown University,
Providence, R. I.

CURRENT NOTES ON PHYSIOGRAPHY.

RIVERS OF SOUTHERN INDIANA.

Certain recent essays that might be gathered under the general title, 'Studies of River Development,' are of interest beyond that which concerns the locality that they treat, inasmuch as they illustrate the degree to which one of the most important divisions of physiographic theory finds practical application.

The 'Drainage of Southern Indiana,' largely outside of the glaciated area, is explained by Newsom (*Jour. Geol.*, Vol. X., 1902, pp. 166–180, map) as 'such as would be logically developed in a country of such combination of hard and soft southwestward dipping strata' as are here found; that is, there are two north-south cuestas formed by the Niagara limestone and the Knobstone standstone, with respect to which the streams are rather systematically arranged, in what seem to be consequent, subsequent and obsequent courses. The author implies an improbably close agreement between the original extent and the present outcrops of certain formations in suggesting that a certain stream, which follows a longitudinal course on weak strata, was deflected into such a course by the sandstones of the next west-lying cuesta when the 'region was first elevated.' The explanation offered for the behavior of one of the master consequents (East White river) in gathering a number of branches from the back (western) slope of the low Niagara cuesta and leading them westward through a notch in the next following Knobstone cuesta, would have been strengthened if it had been presented as exemplifying a type-pattern of drainage well known elsewhere. Indeed, inasmuch as this essay is addressed to professional readers, the essential features of the streams might have been more tersely presented in several instances, had they been named in accordance with a consistent terminology and thus shown to belong to well-recognized classes, rather than described in paraphrases as if they had no relatives elsewhere. The close approach of the Ohio to one of the headwaters of East White river seems to indicate a great and relatively recent increase in the volume of the Ohio, such as has been inferred from other evidence elsewhere.

RIVERS OF SOUTH WALES.

The most notable characteristic of Strahan's 'Origin of the River-system of South Wales' (*Quart. Journ. Geol. Soc.*, LVIII., 1902, 207–225, map) is the neglect of the capture of headwaters of initial consequent streams by the growth of associated subsequent streams along belts of weak strata. It is shown on good evidence that many streams in the Paleozoic area of South Wales pay no attention to the strong east-west folding or to the pronounced north-northwest faulting of the region; and it is reasonably inferred that they were superposed on the previously much denuded Paleozoic area through a cover of Chalk; but in certain localities where the streams follow a northeast-southwest system of disturbances, a late date is given to the disturbances and the streams are made locally consequent upon them. It is recognized that since superposition there has been great denudation, whereby strong relief has been developed appropriate to the resistance of the rocks; but no accompanying adjustment of streams to structures (except in an altogether minor case) is considered, although it is rather clear that a number of captures must have taken place, as in the growth of the Usk headwaters on the Old Red sandstones north of a resistant Carboniferous escarpment and in the associated beheading of several streams south of the Usk. The theory of the adjustment of streams to structures is altogether too well demonstrated to be set aside as 'transgressing the limits of legitimate speculation.' Yet in accordance with the tacit postulate that all rivers are of consequent origin, Strahan reverts to Ramsay's theory of an anticline to form the divide between the Thames and the Severn. Much of the evidence against this obsolescent solution of the Thames-Severn

problem is presented in a somewhat polemical reply by Buckman, entitled 'River Development' (*Geol. Mag.*, Vol. IX., 1902, pp. 366–375).

DISSECTION OF LACCOLITHS.

An ingenious use of physiographic methods has been made by Jaggar in discussing the former size of the laccolith of which the famous butte, Mato Tepee, northwest of the Black Hills, is believed to be a remnant (The Laccoliths of the Black Hills,' 21st Ann. Rep. U. S. Geol. Surv., Pt. III, pp. 163–303). Successive stages in the dissection of laccoliths are summarized about as follows: An early stage produces a dome-shaped hill with radial drainage. One radial stream gains advantage over its fellows and eats out the soft stratum beneath the central portion of the dome; the outward dipping hard beds are undermined and drainage formerly radial outward (consequent) becomes radial inward (obsequent); a former mountain becomes a quaquaversal basin inclosed by a horseshoe ridge. Recession of this ridge and continued erosion on the soft bed uncover a deeper dome of harder rock. Monoclinal shifting of the streams on the soft bed becomes easier than deep cutting into the dome, and thus an encircling (subsequent) valley is developed with a new series of radial streams (resequent) from the stripped mountain core. This alternation from mountain to basin will continue until the igneous mass is discovered; if its upper surface is strongly convex, monoclinical shifting will withdraw the encircling valley from it, leaving an igneous dome with radial ravines; if the upper surface is but slightly convex, the innermost annular streams may be superposed on the laccolith somewhat within its periphery; and still later they may be superposed on the bedded rocks beneath the laccolith. The last condition is thought to occur around Mato Tepee, whose bold column is therefore interpreted to be the remnant of a laccolithic sill about a mile and a half in diameter. W. M. Davis.

SCIENTIFIC NOTES AND NEWS.

Dr. Charles S. Minot, professor of histology and embryology in the Harvard Medical School, was given the degree of Doctor of Science at Oxford University, on the occasion of the tercentenary of the Bodleian Library.

We learn from the *Naturwissenschaftliche Rundschau* that, on the occasion of the jubilee of Abel at Christiania, the honorary doctorate was conferred on the following German mathematicians: Professor Georg Cantor (Halle), Professor J. W. R. Dedekind (Brunswick), Professor David Hilbert (Göttingen), Professor Felix Klein (Göttingen), Professor Leo Königsberger (Heidelberg), Professor H. A. Schwarz (Berlin), Professor Heinrich Weber (Strassburg), Professor Ludwig Blotzmann (Vienna).

At the first autumn meeting of the American Academy of Arts and Sciences, of Boston, Professor Luigi Cremona, of Rome; Professor J. J. Thomson, of Cambridge, England; Professor Emil Behring, of Marburg, and John Morley, Esq., of London, were elected foreign honorary members; and President Hadley, of Yale University, was elected an associate fellow. President Agassiz gave an account of his observations on the coral reefs of the Maldives in the Indian Ocean, and Mr. H. H. Clayton spoke on the observed movements of the dust from the volcanic eruptions in the West Indies and their bearing on theories of atmospheric circulation.

Dr. S. P. Langley, secretary of the Smithsonian Institution, has returned to Washington from Europe.

Mr. Frederick V. Coville, of the Department of Agriculture, has returned to Washington from a botanical expedition to the Klamath country, Oregon. It is understood that a part of his work has been ethno-botanical.

V. I. Jochelson, one of the explorers of the American Museum of Natural History, has arrived at Moscow on his way to New York. He has been making ethnographical studies and collections in the Amur and Yakoust territories for two years.

Professor B. E. Fernow, professor of forestry of Cornell University, has been requested to advise the New York park commissioners as to the best policy to pursue in regard to the trees in Central Park, which are thought to be suffering from lack of sufficient earth.

Mr. Edwin C. Eckel has resigned from the staff of the New York State Museum at Albany to accept a position under the U. S. Geological Survey at Washington.

Kiichi Miyake, Ph.D. (Cornell, 1902), has recently received an appointment from the government of Formosa for two years' travel and study in Europe. Dr. Miyake is a graduate of the Doshisha College in Japan, afterwards spending four years at the Tokyo Imperial University. He entered Cornell University in September, 1899, where he spent two years in continuing his graduate work, giving especial attention to fertilization and embryology in the Phycomycetes and in the Abietinæ. He sailed from New York for Bonn on October 7.

Dr. Max Proebst has been advanced, by royal decree, from the position of director of the Royal Statistical Bureau of Bavaria to that of an independent chief directorship, and the Order of Merit has been bestowed on him in recognition of his services. Dr. Karl Trutzer now assumes the position formerly held by Dr. Proebst.

King Oscar, of Sweden, has bestowed the Grand Cross of St. Olaf on Capt. Otto Sverdrup, the arctic explorer, and has given him an annual allowance of $800.

The eightieth birthday of John Fritz, ironmaster and inventor, of Bethlehem, Pa., will be celebrated by a dinner given in his honor at the Waldorf-Astoria, in the ballroom, on Friday, October 31. The dinner will also signalize the founding of the John Fritz gold medal, for achievement in the industrial sciences, the medal to be awarded annually by a committee of members of the American Society of Civil Engineers, the American Society of Mechanical Engineers, the American Institute of Mining Engineers and the American Institute of Electrical Engineers. The organizing committee having the matter in charge on behalf of these societies has already raised $6,000, representing the contributions of some 500 members of the engineering professions in this country and in Europe. The medal has been entrusted to the American sculptor, Victor D. Brenner.

Mr. Jacob Richards Dodge, connected with the department of agriculture from its organization in 1862 until he retired in 1893, and known for his contributions to agriculture and statistics, died at Woburn on October 7, at the age of seventy-nine years.

The death is announced of two well-known American physicians: Dr. John Bryne, past president of the American Gynecological Society, and Dr. Abel M. Phelps, past president of the American Orthopedic Society.

Dr. John Hall Gladstone, F.R.S., known for his researches on chemical combinations and the relations of chemical and optical science, formerly professor of chemistry at the Royal Institution, died on October 7, at the age of seventy-five years.

Dr. B. J. Stokvis, professor of pharmacology and general pathology at the University of Amsterdam, died on September 29, at the age of sixty-eight years.

Dr. Jean Habel, of Berlin, known for his explorations in the Andes and in Canada, died on September 11.

M. Vincent Leche Chesnevieux, the French traveler and geologist, has died at the age of eighty-six years.

The twelfth annual meeting of the Ohio Academy of Science will be held at Columbus, November 28 and 29. Members desiring to present papers are requested to send titles and time required to the secretary on or before November 1.

At the first meeting of the Geological Conference of Harvard University, informal reports were made by officers of the Division of Geology on their summer work. Professor Shaler spent part of the summer in Alaska, noting especially the mountain forms and fiords of our northwestern coast. Professor Davis, accompanied by two advanced students, made an excursion through southern Utah and northern Arizona, visiting the Colorado Canyon at Toroweap valley, and making special study of the Hurricane fault; he afterwards examined some of the Basin ranges and the Tertiaries at Green river. Professor Wolff completed the Franklin folio, New Jersey, for the U. S. Geological Survey, and

continued his field work on the ancient crystalline rocks of Berkshire county, Mass. Professor Smyth made a brief visit to the Lake Superior district, and then went to Colorado, where he made an extended reconnaissance of the mining camps on the Yukon and at Cape Nome. Professor Jackson and Mr. Cushman spent some time collecting fossils in the Helderbergs and Catskills of eastern New York. Professor Woodworth continued his work for the N. Y. State Geological Survey on the glacial geology of the Hudson and Champlain valleys and around the northern side of the Adirondacks. Professor Palache was engaged on office work following field studies of a year ago on the geology of Bradshaw mountains, Arizona, for the U. S. Geological Survey. Dr. Jaggar went to Martinique and St. Vincent in May on the U. S. relief ship, *Dixie*, and remained in the West Indies until the end of July. Mr. Raymer conducted a summer course for students in mining, making practical study of mines and works in and about Denver, Silverton, Telluride, Leadville and Salt Lake City. Mr. White led a party of geological and mining students through southern Colorado, visiting La Plata mountains, Animas Canyon and the San Juan district; after the party disbanded, Mr. White examined various mining and reduction plants in Colorado and Utah.

AN Intercollegiate Geological Excursion, similar to the one a year ago at Westfield, Mass., in which six colleges and as many preparatory secondary schools were represented by forty-six participants, is proposed for Saturday, November 1, under the leadership of Professor B. K. Emerson, of Amherst College. The party will gather on Friday evening at the Cooley House, Springfield, Mass. On Saturday morning the 8:30 train will be taken to Holyoke, and the day will be spent on the Mount Tom trap range, returning to Holyoke in time for evening trains in all directions. The chief features to be seen are the structure of the Triassic trap sheets and sandstones; contacts of the trap with the underlying and overlying sandstones; fossil footprints in the sandstones, glacial deposits and terraces along the Connecticut river. Teachers and students of geology who desire to join the excursion are requested to communicate with Professor Emerson not later than October 26.

DR. F. L. RANSOME has just completed a comprehensive report on the geology and ore deposits of the Globe copper district, Arizona, for the United States Geological Survey. The region is dissected by a remarkable network of faults, of various geologic ages, and the occurrence of the ores is related to some of the older of these fissures. The copper ores hitherto mined in the district have been oxidized and are consequently free from sulphur, but the exploitation of the deeper sulphide ores is yet in its infancy. The district has produced in the neighborhood of 120,000,000 pounds of copper. The greater part of this output has come from the Old Dominion mine, which has for years been working large bodies of oxidized ore found in limestone occurring by the side of a strong fault. During the present season Dr. Ransome is to continue the investigation of the copper deposits of Arizona by undertaking a detailed geologic study of the Bisbee district, in which is the well-known Copper Queen mine.

UNIVERSITY AND EDUCATIONAL NEWS.

THERE will be erected this year for Wesleyan University a physical laboratory, given by the alumni. It is expected that this and a new college hall will be ready for dedication in July, 1903, when the college will celebrate the tercentenary of the birth of John Wesley. It is also announced that a new astronomical observatory will be erected at a cost of $40,000, the money having been provided by a brother of Professor J. M. Van Vleck, professor of mathematics and astronomy and vice-president of the University.

AT the recent meeting of the board of trustees of Columbia University it was announced that $7,500 had been given by citizens of New York to support the chair of social and political ethics, to which Dr. Felix Adler has been called. $10,000 has been given anonymously for the purchase of books for the library and $1,300 has been given by Mr. J. H. Hyde and Mr. F. R. Coudert, Jr., for two scholarships

for students studying in France, the arrangements for which we have already announced.

PRESIDENT HARPER has announced that plans are being made for a school of technology as part of the University of Chicago.

THE Hon. Carroll D. Wright, U. S. Commissioner of Labor, was installed as president of the new collegiate department of Clark University on October 9. Addresses were made by Senators Hoar and Lodge and by Dr. Hall, president of the University. President Wright made an address on the relations between college training and citizenship after having outlined the purposes of the new college, which, he said, opened auspiciously with an entering class of seventy-nine students. It is expected that President Wright will take up his residence in Worcester in about two years.

THE inauguration of Dr. Frank Strong, formerly of the University of Oregon, as chancellor of the University of Kansas, will take place on Friday, October 17. On the Thursday afternoon preceding there will be a meeting of the Kansas City Section of the American Chemical Society at Lawrence, with the reading of papers, and in the evening Dr. Harvey W. Wiley, of Washington, D. C., will deliver the address of dedication of the chemistry building, his subject being 'The Rôle of Chemistry in University Education.' At the inauguration exercises President Arthur Hadley, of Yale, Chancellor Strong, Governor Stanley, Regent Scott Hopkins, President Murlin, of Baker University, Principal Whittemore, of Topeka, Professor W. H. Carruth, A. C. Scott and others will participate. In the evening it is proposed to have an inauguration luncheon in the new natural history museum, which is nearly completed, with after-dinner speeches by numerous college presidents and educators.

THE Rev. Dr. G. M. Ward has resigned the presidency of Rollins College, Winter Park, Florida.

DR. D. W. HERING, professor of physics in New York University, has been elected dean of the graduate school.

DR. JOHN H. HAMMOND, recently appointed professor of mining at Yale University, will not reside at New Haven.

AT Yale University, Dr. Milton B. Porter has been promoted to an assistant professorship in mathematics, and Dr. William R. Coe to an assistant professorship in anatomy.

W. G. CADY, Ph.D. (Berlin), now in the Coast and Geodetic Survey, has been appointed to an associate professorship of physics at Wesleyan University, vacant by the resignation of Professor E. B. Rosa, to accept a position in the Bureau of Standards.

AT the State School of Mines, Golden, Colo., Mr. C. W. L. Filkins, of the engineer's staff of Cornell University, has been appointed professor of civil and mining engineering. Mr. H. C. Berry has been appointed instructor in algebra and field surveying, and Mr. E. W. Gebhardt has been appointed instructor in trigonometry and analytical and descriptive geometry. Professor W. C. King, of the Montana School of Mines, has been appointed professor of a new chair, metallurgy and mining. Professor King will not begin his work until about the middle of the year.

IN the University of Michigan Dr. M. Gomberg has been advanced to the rank of junior professor of organic chemistry.

DR. HAROLD PENDER, Ph.D. (Johns Hopkins, 1901), has been appointed instructor in physics in Syracuse University. In the same department, Dr. Frederick A. Saunders, Ph.D. (Johns Hopkins, 1899), formerly instructor, has been made associate professor.

MR. WILLIAM A. HAMILTON, of Chicago University, has been appointed to the instructorship in astronomy and mathematics at Beloit College, left vacant by the resignation of Professor George Bacon, who has been called to the chair of physics in Worcester University.

THE chair of hygiene at McGill University, vacant by the death of Dr. Wyatt Johnston, has been offered to Dr. E. A. Hankin, bacteriologist to the government of India, but has been declined by him.

DR. JOHANNES ORTH, professor of pathological anatomy at the University of Göttingen, succeeds the late Professor Virchow in the chair of pathological anatomy at the University of Berlin.